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Doctoral Thesis

Government Action under Constraints

Fiscal Development, Fiscal Policy and Public Goods Provision during the Great Depression and in 19th and early 20th Century Brazil

Author:
Andrea Papadia

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Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it). The copyright of this thesis rests with the author. Quotation from it is permitted, provided that full acknowledgement is made. This thesis may not be reproduced without my prior written consent. I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

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Abstract

This dissertation is composed by three papers whose unifying themes are the origin and impact of fiscal institutions. The main contribution of this paper is twofold. First, it highlights the usefulness of the concept of fiscal capacity for the macroeconomics and international finance literatures by demonstrating its impact on sovereign default and fiscal dynamics during the Great Depression. Limits to the ability to tax have clear implications for macro-financial research, but are neglected by much of the literature. Second, my work contributes to the fiscal and state capacity literature by focusing on municipal level fiscal institutions in Brazil. Although research in this field is burgeoning, our understanding of the origin and impact of fiscal institutions in many parts of the world, including Latin America, is still very limited, particularly at the sub-national level. In terms of structure, the dissertation is a backwards journey from the impact of fiscal institutions to their origin. The first paper studies one of the ultimate outcomes of fiscal dynamics – sovereign default – by analyzing the debt crisis of the 1930s. The second paper takes the collapse in public revenues during the Great Depression as a starting point and demonstrates that fiscal institutions were a fundamental factor in the dynamics of fiscal aggregates. By shifting the focus to a single country and a different time period – the second half of the 19th and the early 20th centuries – the third paper demonstrates that slavery was deeply detrimental to the development of local governments' ability to tax and provide fundamental growth and welfare-enhancing public goods in Brazil.

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This dissertation is the result of four years of work, but its roots are much deeper. I have been interested in history for as long as I can remember, but my love for economics flourished well into my Bachelor degree at the University of Rome "Tor Vergata". After a year and a half asking myself if I had made the right choice in picking economics as my main field of studies, I took the Economic History course taught by Stefano Fenoaltea. I cannot overemphasize how much this broadened my horizons and allowed me to truly appreciate the intellectual challenge posed by economics and economic history. Another fundamental step in my formation was the course on European Economics taught by Alessandro Piergallini, which I took in the last year of my bachelor degree. This the the course that showed me, for the first time, the true power of economic theory as a way to generate and sharpen insights, and as a guide to policy making and the design of institutions.

In retrospect, I cannot imagine a better way I could have continued my formation than the International, Trade, Finance and Development Masters at the Barcelona Graduate School of Economics in which I enrolled at the end of my first degree. There, I had many wonderful teachers, but Fernando Broner and Jaume Ventura stand above all in my personal experience. Their approach to fundamental questions in Macroeconomics and International Economics has deeply influenced my own work. On top of this, Fernando has offered me invaluable support and guidance over the coming years and has played a big role in the next sep in my career as Max Weber Fellow at the European University Institute. I have wonderful memories of all the colleagues with whom I shared this fantastic year in Barcelona, but would particularly like to thank Armando, Carla, James, Jocco, Monica, Moritz, Óttar, Saffet, Selim and Tomás.

Although, by this time I had become completely fascinated by economics, the allurement of history was still present in my mind. The next step in my formation was to come to the LSE for a research masters first, and a PhD later, both in Economic History. The LSE and the Department of Economic History are the places that taught me what

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Chapter 1

Introduction

This dissertation is composed by three papers whose unifying themes are the origin and impact of fiscal institutions. For a long time, economists and economic historians have thought about taxation, and the power of states more generally, in a negative light. Research focused on the constraints imposed on the executive, which helped to protect the property rights of citizens and allowed enterprise, trade and prosperity to flourish. In other words, the object of enquiry was identifying how societies limit the predatory power of the state. In the last two decades, economic historians and economists have slowly embraced the idea – which has long history in other social sciences, particularly historical sociology – that weak states can hinder growth and prosperity as much as unconstrained ones. The role of states in sustaining institutions, maintaining order, protecting rights and providing essential public goods has been fully recognized. More importantly, however, it has been recognized that these basic functions are not to be taken for granted. States acquire their capacities through complex and non-linear processes that often stretch over centuries. Historical research has been instrumental in drawing attention to this fact. Armed with these insights, economists and development practitioners have also reevaluated the experiences of today's developing countries. The spotlight has shifted from the predatory action of states, to their powerlessness to enforce rules, provide public goods and sustain institutions.

This dissertation follows in the spirit of this work. It does so by applying these ideas to areas in which the deeply-rooted constraints to the fiscal capacity of states have largely been ignored – fiscal policy and sovereign default during the Great Depression – and by exploring an under-studied context – that of local institutions in late 19th and early 20th century Brazil.

1.1 Overall contribution

The main contribution of this work is thus twofold. First, it highlights the relevance and usefulness of the concept of fiscal capacity for the macroeconomics and international finance literatures by demonstrating its impact on sovereign default and fiscal dynamics during the Great Depression. Fiscal capacity is a widely recognized cornerstone in historical and development research, but as Besley and Persson (2009, 2014) point out, states' ability to tax is still mostly taken for granted in the rest of the literature. At the same time, limits to the ability to tax have clear and fundamental implications for any macro-financial research involving the public sector. Understanding the origin and impact of fiscal institutions is thus of fundamental importance, and not only for developing countries, as the recent European debt crisis has shown. Second, although the fiscal and state capacity literature is an exciting and prospering field, most of the work is still focused on Europe and its offshoots north of the Equator, and on East Asia. Our understanding of the origin and impact of fiscal institutions in many parts of the world, including Latin America, is still very limited, particularly at the sub-national level (Hoffman, 2015; Nafziger, 2016; Koyama and Johnson, 2017). Therefore, my paper contributes to this literature by focusing on municipal level fiscal institutions in Brazil.

In terms of structure, this dissertation is a backwards journey from the impact of fiscal institutions to their origin. The first paper studies one of the ultimate outcomes of fiscal dynamics – sovereign default – by analyzing the debt crisis of the 1930s. This was the, arguably, largest and most widespread sovereign debt crisis in history. In my analysis, I find that the collapse in public revenues that hit a number of countries during the Great Depression played a fundamental role in the default of both national and sub-national governments across the globe. The second paper takes the collapse in public revenues during the Great Depression as a starting point and demonstrates that historically-determined fiscal institutions were a fundamental factor in the dynamics of fiscal aggregates. The paper also offers evidence on the mechanisms that shaped the development of these institutions. Finally, the last paper uses a unique case study to delve deeply into the historical origins of fiscal institutions. By shifting the focus to a single country – Brazil – and to a different time period – the second half of the 19th and the early 20th centuries – this paper demonstrates that slavery was deeply detrimental to the development of local governments' ability to tax and provide fundamental growth and welfare-enhancing public goods, such as education and infrastructure.

One of the defining characteristics of this work is the attention it devots to local level outcomes. This is most direct in the paper on municipal institutions in Brazil. The paper on the interwar debt crisis also tackles sub-national administrative units directly by investigating the predictors of default at the municipal level, alongside those specific to national-provincial governments. The paper on fiscal dynamics focuses on central governments. However, the relationship between local and central governments is at the heart of one of the mechanisms for the development of fiscal capacity, which I exploit in the analysis.

Additionally, all three papers feature extensive data collection at the local level. For the paper on Brazil, I use various sources – such as censuses and statistical yearbooks – to gather data on the characteristics of municipalities. For my paper on the debt crisis, I produce new estimates of the debts of sub-national public bodies by putting together multiple scattered sources. The result is a much more detailed picture of public debts in the interwar period than that provided by any other existing dataset. For my paper on aggregate fiscal dynamics, I have collected data on tax revenues for local governments, whenever possible, in order to assess the evolution of fiscal capacity in the interwar period in an holistic way.

1.2 Motivation

The three papers in this dissertation grapple with some of the most important economic and social phenomena of the last two centuries. Unsurprisingly, these have sparked much research and heated debates amongst economic historians, economists and other social scientists in the past. They continue to be fertile fields of enquiry today.

More than 20 years after Ben Bernanke coined the expression (Bernanke, 1995), understanding the Great Depression remains the "Holy Grail of Macroeconomics". Even the recent crisis has not been able to dethrone the Depression as the deepest and most widespread peace-time economic slump in history. Much like the Holy Grail, a complete and entirely satisfactory explanation for the Depression still appears to be out of reach, if not entirely mythical, given the complexity of the event and the disparate experiences of countries around the world.

A consensus regarding the crisis has gathered around Barry Eichengreen's masterful "Golden Fetters" (Eichengreen, 1992), which placed the interwar Gold Standard square at the centre of explanations regarding the devastating effects of the crisis on the world economy, and whose main message is in line with the views expressed by other giants

of the professions, such as Peter Temin (Temin, 1989), Ben Bernanke and Harold James (Bernanke, 1983; Bernanke and James, 1991).[1] Eichengreen's synthesis relied on important previous work with Jeffrey Sachs (Eichengreen and Sachs, 1985, 1986) and classic works by Nurkse (1944) and Kindleberger (1973), amongst many others. Eichengreen and Temin (2013) have recently restated their classic arguments in connection to the recent European crisis.

However, some questions remain unanswered or only partially answered. For example, to what extent were monetary forces directly responsible for the Depression?[2] What is the role of the 1929 Wall Street Crash? What determined the outbreak and diffusion of financial panics and bank failures? Could different fiscal policies have averted the worst of the slump? How do we rationalize the dramatically different experiences of countries during the Depression?

Recent research has shown that much can be learned by breaking down the Depression into its many different aspects and/or country-specific experiences, on the condition of retaining the international outlook advocated by Eichengreen and Bernanke, and by taking the connections between different sectors of the economy into account. For example, recent work has made important steps forward into clarifying the mechanisms for the diffusion of the Depression through financial channels across countries (Accominotti, 2012; Ritschl and Sarferaz, 2014).

In addition, while it is generally recognized that the Great Depression provided contemporary policy-makers with fundamental lessons for responding to the recent crisis, it is also the case that the latter has brought new aspects of the Depression into the spotlight. The role of mortgages in bank failures highlighted by Postel-Vinay (2016) and the impact of interbank markets in straining the US's banking sector outlined by Mitchener and Richardson (2013) are examples of these. In this dissertation, I set out to help answer some of the remaining open questions of the Great Depression by focusing on two specific aspects, which also happen to resonate with recent events.

My last paper departs from the Great Depression to tackle the historical origins of fiscal capacity and of its close companion, public goods provision. It does so for a country that has been hailed as the nation of the future for most of its independent history, but

[1] In earlier work, Temin had played down the role of monetary forces in the Depression highlighting, instead, an autonomous fall in aggregate demand as the most important factor (Temin, 1976).

[2] Once again, after more than 20 years, Bernanke (1995)'s assertion still stands: there is convincing evidence that monetary shocks caused the depression by affecting financial stability and real wages, but we cannot say we fully understand the Depression until we figure out why nominal adjustment in wages did not take place.

whose growth performance has been consistently thwarted by institutional shortcomings, of which the recent massive corruption scandals are just the latest manifestation: Brazil.

The paper studies municipal institutions, which are important players in Brazil's administrative landscape today. They raise a substantial share of public revenue – around 10-15%, a similar percentage to that of my period of analysis in the early 20th century – and are responsible for an even greater share of expenditure thanks to transfers from the federal government. Some of their most important responsibilities include the provision of public education, today as in the past. I link the historical evolution of these institutions to another fundamental feature of Brazilian history: slavery.

The publication of Stanley Engerman and Robert Fogel's controversial "Time on the Cross" in 1974 (Engerman and Fogel, 1974) and the intense debate that followed it have established slavery as one of the classic themes in economic history. Recent debates on the role of slavery in the industrial revolution and in the rise of capitalism attest to the continuing interest around this topic. However, existing research has not yet fully fleshed out the legacy of slavery in a systematic way, particularly its effect on broad economic and institutional outcomes. This is especially true for Brazil, the largest importer of slaves during the Atlantic slave trade. Although attempts have been made to link slavery to institutional and economic developments in the country, these mostly offer indirect evidence or are altogether inconclusive. My work offer prima facie evidence on the legacy of slavery by exploring its impact on local fiscal institutions.

In the next section, I discuss some further motivating factors for my work. In doing so, I also highlight parallels and lessons that the historical experience offers for today.

1.2.1 Fiscal fragility and debt crises

As mentioned, several aspects of the recent crisis have parallels in the Great Depression. The 1930s debt crisis, which I study in my first paper, has clear similarities with the European debt crisis, for example, and comparisons between Germany's situation in the 30s and that of Greece in the recent past have drawn particular attention in the media. Academics have also taken part in the debate: see for example the back and forth between Albrecht Ritschl and Hans-Werner Sinn on The Economist (Ritschl, 2012b). Although the recent crisis is not comparable in size and scope with the Great Depression, the two share some key characteristics. Both took place within a fixed exchange rates regime – the Gold Standard in the 1930s and the Euro in the 2010s – and both helped to fuel the rise of populist and extremist parties.

Both crises have also been characterized by, at best, imperfect coordination and cooperation between countries. The European Union and the Eurozone's institutions offer a much more structured environment for the resolution of crises compared to the League of Nations and the Bank for International Settlements in 1930s, and collaboration between central banks since 2007 has been a high point in the history of international policy cooperation. The consequences of the recent crisis would have presumably been substantially more devastating under the institutional setting of the interwar period. Nonetheless similar problems emerged in both crises. The four clearest and most important are: 1) excessive borrowing and growing imbalances in some countries 2) the lack of a decisive and rapid restructuring of unsustainable debts, 3) the (mostly) self-defeating austerity imposed on trouble countries, and 4) the unwillingness of surplus countries to reflate and assist the adjustment of deficit countries. The Greek experience stands testimony to these failures.

Although the debt crisis of the 1930s was widely discussed by contemporaries and has been studied by scholars thereafter, one of its most striking features – the vastly different dynamics of public revenues across countries – has largely been neglected by the literature. As in other aspects, the fiscal troubles of European countries today pale in comparison to their interwar counterparts. I put this issue front and center in my work and show that the collapse in tax revenues was the most reliable predictor of sovereign default in the interwar period, above and beyond other indicators of macroeconomic health. This finding raises some interesting issues regarding sovereign debt crises in general. In particular, it speaks to recent work focusing on the determinants and potential endogeneity of fiscal limits (Bi, Shen, and Yang, 2016; Arellano and Bai, 2016) and the role asymmetric information in defaults (Catão, Fostel, and Ranciére, 2011, Forthcoming).

1.2.2 Fiscal policy under constraints: fixed exchange rates, fiscal development and austerity

As discussed, the interwar period was characterized by a large volatility and procyclicality in fiscal aggregates. However, the collapse in public revenues experienced by a number of countries during the Depression is generally considered as a simple byproduct of the slump and, in the more careful accounts, as a consequence of the reliance of some countries on narrow sources of revenues, such as trade taxes and commodity royalties. The determinants of the dynamics of government financing have thus not been systematically explored.

There are many ways in which these phenomena, which are the topic of the second paper of this dissertation, can negatively affect the economy. Some of these are not tied to the use of fiscal policy as a tool to actively fight business cycle downturns. They are rather related to political economy considerations and to the functioning of states. I discuss these in detail in the paper. Here, however, I want to stress the connection between revenue cyclicality, procylical fiscal policy and austerity to further highlight the parallels between the Depression and the recent crisis.

In a tax-smoothing framework, optimal fiscal policy is acyclical: during a slump tax revenues should decrease in line with output, with expenditure remaining stable, while the opposite should take place during a boom (Barro, 1979; Lucas and Stokey, 1983).[3] In a Keynesian framework, instead, fiscal policy is meant to be countercyclical: in a slump tax revenues should go down and/or government expenditure should increase leading to deficits and aggregate demand stimulus. In either case, borrowing should be used to make up for the funding shortfall. During the Depression, tax revenues and other government income did indeed collapse in unison with output. However, borrowing did not generally compensate for the fall in revenues leading to large falls in expenditure as well, in contrast to the recommendations of both the Keynesian and tax-smoothing frameworks. In its extreme manifestations, this contributed to outright austerity and deflationary pressures. Some countries attempted to stem the tide of falling revenues by increasing tax revenues in an even starker manifestation of austerity.

The recent crisis has offered the opportunity to assess the impact of austerity on the economy. Its potentially self-defeating nature has been made evident most concretely in the case of Greece, which has experienced a Depression level contraction in its output. Research has highlighted that, through hysteresis, austerity can also have negative effects beyond its impact on the severity of the business cycle. By expanding on the approach of Blanchard and Leigh (2013), Fatás and Summers (2016) and Gechert, Horn, and Paetz (2017) show large costs of negative fiscal shocks – increases in tax revenue or decreases in expenditure – in the recent crisis, with considerable growth effects in the medium/long-run. There is furthermore evidence that tax evasion and corruption, the advanced-country-equivalents of low fiscal capacity, have contributed to making the costs of fiscal consolidations particularly grievous in countries like Greece (Pappa, Sajedi, and Vella, 2015).

The Gold Standard is generally seen as the principal impediment to policies which

[3]This result assumes that expenditure is exogenously determined. See Ferriére and Karantounias (Forthcoming) on how this can change when government expenditure is endogenized.

could have helped fight the Great Depression. The seeds of the Gold Standard debacle, according to this view, were already evident in the the run-up to the Depression. With specific regard to fiscal policies, Eichengreen (1992) stresses that these became highly politicized after World War I eliminating the guarantee that measures would be taken to adjust the balance of payments as was, instead, the case during the classical Gold Standard. This undermined the credibility of the whole system. The loss of credibility also made domestic policies less effective by exposing countries to the moods of financial markets and the volatile expectations of investors. As a result of this, Eichengreen argues, international cooperation became more important. However, this was made difficult by tensions due to war debts and reparations and to different conceptual frameworks prevailing in different countries.

Eichengreen further argues that leaving the Gold Standard was a necessary, but insufficient condition for the recovery. While leaving the Standard freed up resources for monetary and fiscal policy, equally important was abandoning the ethos and financial orthodoxy that characterized the rules of the game.[4] Bernanke and James (1991) argue that differences in performance in 1929-30 were negligible between countries that left gold during the first wave of devaluations in 1931 and those that did not. Instead, Eichengreen ascribes the decision to leave gold and to run more or less expansionary policies to internal politics and to the economic events of the early 1920s, particularly the inflationary and hyper-inflationary experiences of countries such as Italy, France, Germany and Austria. In turn these experiences were rooted in the upsetting of the pre-WWI consensus regarding the distribution of income and fiscal burdens, which eventually led to countries relying on the printing press to finance deficits.

Even in countries with no recent history of severe inflation, however, fiscal policy was generally very conservative, even after the abandonment of the gold anchor. In the United States, for example, large fiscal deficits were not employed to fight the slump. Increases in government expenditure, which accompanied Franklin D. Roosevelt's New Deal, were bundled with increases in taxes. Moreover, small deficits at the federal level were compensated by surpluses at the state level (Fishback, 2013). The United Kingdom, the first country to leave the Gold Standard and the poster child for the benefits of shedding the golden fetters, also did not engage in a distinctly expansionary fiscal policy (Middleton, 2013). The role of fiscal policy in Germany's strong recovery from the Depression in the

[4]The positive effects of abandoning the gold peg were naturally potentially weakened by beggar-thy-neighbour policies in the context of competitive devaluations, but Eichengreen argues that generalized devaluaitions still would have had positive effects by reflating the economy.

second half of the 1930s was also negligible (Ritschl, 2002a). In general, the idea that fiscal policy could be a tool to fight the slump was considered distinctly exotic (Almunia, Bénétrix, Eichengreen, O'Rourke, Rua, Tenreyro, and Perri, 2010) and this attitude was not restricted to the traditional strongholds of financial orthodoxy. The 1934 volume edited by UK's Labour economist Hugh Dalton entitled "Unbalanced Budgets" (Dalton, 1934), for example, treats budget deficits as the unavoidable symptom of a malady: the Depression. Similar attitudes were found in other unlikely quarters such as the communists in France (Eichengreen, 1992). However, even if budget deficits were not contemplated as a tool to fight the Depression, smaller contractions in government financing and expenditure would, at a minimum, not have aggravated the economic and political turmoil of the 1930s.

Doubts remain on whether Gold Standard membership was the only or even the most important constraint to running a more expansionary or, at the very least, less procyclical fiscal policy. Germany, for example, faced a borrowing constraint due to large debts and war reparations. Moreover, debt monetization was constrained by international agreements (Ritschl, 2013). Although these factors were connected to the preservation of the fixed exchange rate regime, they were also specific to the country. More generally, it is uncertain that many countries would have been in the position to run effective and credible fiscal policies just by leaving the Gold Standard. Even in the UK, an expansionary fiscal policy might have led to a sharp increase in borrowing costs (Middleton, 2013). Thus, while not disputing the general consensus that the Gold Standard played a pivotal role in the Depression, it is crucial to investigate what other constraints countries were facing.

In order to spend more, countries faced three non-mutually-exclusive options: 1) borrowing on financial markets, 2) monetizing the debt 3) using financial repression to channel resources to the public sector. Financial repression was indeed used in a number of countries, including Germany, but mostly in the second half of the 1930s and in connection with rearmament and growing economic nationalisms. Moreover, it was particularly viable in non-democratic and increasingly authoritarian countries. Monetizing the debt was certainly a possibility for countries no longer on gold. However, the size of the deficits some countries would have had to finance by borrowing exclusively from central banks would have potentially led to capital flight, at least in the absence of extensive capital controls, with important negative economic consequences. Borrowing from financial markets was also not a simple option. After 1929, international financial markets were highly dysfunctional and could not provide a reliable source of financing. In some countries, domestic financial markets were underdeveloped, while in others wealth and savings had been wiped

out by hyperinflations in the 1920s and the financial crash of 1929-31. Nonetheless, borrowing on domestic financial became increasingly important as the Depression progressed. As I discuss in the paper, the domestic share of public debts increased from 50% in 1927-29 to 60% in 1933-38. And countries that successfully managed to smooth their revenues were able to do so predominantly through borrowing.

In the paper, I show that their ability to do so depended crucially on their degree of fiscal development: the fiscal capacity with which countries entered the Great Depression was a key determinant of how the slump translated into the loss of public revenues. Moreover, I show that fiscal development was more important for countries off gold than for those that remained tied to it. This suggests that the decision to leave the Gold Standard might have been endogenous to fiscal capacity. In other words, countries might have left the Gold Standard because they were in advantageous position to do so thanks to the strength of the their institutions.

These considerations are also potentially valid for the current European crisis. Could countries like Greece, Ireland, Portugal, Spain, and Italy have run more countercyclical fiscal policies had they been outside the Eurozone or not tied to the influence of the *troika*? Given the precarious situation of their public finances and/or banking sectors, this seems unlikely, at least without external help and/or international policy coordination. Both the former and the latter, however, would have been even less forthcoming in the absence of European institutions and the common goal of preserving the Euro.

1.2.3 Fiscal capacity, public goods and the shadow of slavery

In the third paper of this dissertation, I outline in detail how slavery affected the development of local institutions in Brazil. Here, I briefly discuss the literature on the relationship between coerced labour, inequality and development in the Americas (and beyond), in order to motivate and set the stage for the more focused discussion in the paper.

The work of Stanley Engerman and Kenneth Sokoloff (Engerman and Sokoloff, 1997, 2012) has been instrumental in shaping research on slavery in the Americas by tying it to economic and political inequality. In a reformulation of the classic "Staple Thesis", the authors argued that different initial endowments led to different types of colonization across the continent. In particular, countries with land suitable for plantation agriculture developed production systems based on large scale land ownership and slave labour. This gave rise to high levels of inequality and adverse institutional developments which constrained growth compared to other parts of the continent. Daron Acemoglu, Simon Johnson and

James Robinson exploited Engerman and Sokoloff's basic premise that different types of colonization have shaped long-term development in their seminal 2001 paper (Acemoglu, Johnson, and Robinson, 2001). Despite some serious flaws in its identification strategy and data (Albouy, 2012), this work has spawned a huge and influential literature on the role of history and institutions in economic growth.

Engerman and Sokoloff's basic tenet that endowments might affect institutional development through the channels of colonization, slavery and inequality has been contested – or only partially supported – from both a conceptual (North, Summerhill, and Weingast, 2000; Coatsworth, 2005) and empirical (Nunn, 2008a; Williamson, 2010; Bruhn and Gallego, 2012) standpoint. Moreover, the search for a negative relationship between economic inequality and long-term development has produced a contradictory and inconclusive body of research.

The attempt to establish a link between political inequality and deficient long-term development has been more successful; Acemoglu, Bautista, Querubín, and Robinson (2008)'s study of Colombia's region of Cundinamarca offers an early example of this recent wave of work. Bruhn and Gallego (2012) offer a systematic overview of the legacy of different types of colonial activities in the Americas, which also supports the importance of political inequality. They characterize colonial activities as "bad" – those based on increasing economies of scale – "good" – those not characterized by increasing economies of scale – and "ugly" – those based on the exploitation of forced labour. The authors find that "bad" activities are linked to worse economic outcomes today, while the evidence for "ugly" activities is much weaker. The authors fail to find a connection between these activities and economic inequality and showed, instead, that political institutions and political representation were the channels through which they affected development. Single country studies with sophisticated identification strategies, however, have highlighted that "ugly" activities based on forced labour also had adverse developmental consequences, which mostly worked through political channels as well (Dell, 2010; Acemoglu, García-Jimeno, and Robinson, 2012).

In the Brazilian context, and more specifically for the province/state of São Paulo, William Summerhill has performed a thorough analysis of the impact of different types of inequality and of slavery (Summerhill, 2010). The author's findings highlight the lack of a robust relationship between any of these indicators and long term development. Funari (2017) successively applied a similar approach to Minas Gerais, Pernambuco and Rio Grande do Sul, finding no relationship between political inequality and development. For

economic inequality, instead, the relationship appears to vary across states, presumably because of their different colonial experiences.[5] Reis (2017) confirmed Summerhill's finding of the lack of relationship between slavery and long term growth for the whole of Brazil.

In my work, using a different identification strategy, I establish that slavery had a negative impact on crucial developmental outcomes: fiscal capacity and public goods provision. Moreover, I highlight a channel for this effect that depends on neither economic nor political inequality strictly defined. My findings suggest that slavery affected the development of local institutions by shaping the settlement of foreign immigrants during the second half of the 19th century. Political representation is certainly an important part of the story. However, it was the ability of migrants to "vote with their feet" and exert a political pressure well beyond their numbers due to Brazil's labour shortage – rather than influence exerted through formal political institutions, which are the standard measures of political inequality – that helped them shape local institutions. The paper also supports an interpretation of the determinants of development in Latin America outlined by North, Summerhill, and Weingast (2000) and Coatsworth (2005), which questions a direct and unbroken link between colonial settlement and long-run economic growth, and highlights the importance of political and economic developments in the 19th century.

1.3 Structure of the dissertation

The goal of the remainder of the introductory part of this Dissertation is to further set the stage and motivate the three research papers. In Chapter 2, I offer a detailed literature review in which I discuss existing research related to my work. There, I outline the lessons present in the various strands of the literature and identify the remaining gaps in our understanding. Concomitantly, I illustrate how my own work contributes to filling some of these. I begin by reviewing the literature on the central theme of this dissertation: fiscal capacity (Section 2.1). I outline the definition of this concept, how it is commonly measured and what impact we can expect it to have on economic outcomes. In Section 2.2, I discuss the rich literature on sovereign debt crises drawing on research on the Great Depression, other time periods, and theory. The final strand of literature I discuss relates to slavery and development in Brazil (Section 2.3). After presenting the three papers (Chapters 3-5), I provide my overall conclusions (Chapter 6).

[5]The author also found an ambiguous relationship between political and economic inequality.

Chapter 2

Literature review

2.1 Fiscal capacity: definition, measurement and impact

2.1.1 What is fiscal capacity and why does it matter?

Despite a growing recognition of the role of fiscal institutions in shaping economic performance, most macroeconomic models still assume that governments always have the ability to raise the desired or needed tax revenues and are, in general, *effective*. This was clearly not the case historically, but neither is it today in many developing countries. Historians, sociologists and other social scientists have long recognized that the development of means to raise taxes deserves serious study. Joseph Schumpter argued along these lines already in 1918 in the wake of the tumultuous changes brought about by World War I (Schumpeter, 1918). A relatively recent literature in modern Economics has also started tackling fundamental questions, such as where states' ability to raise revenues comes from and what impact it has on the economy, beyond the distortionary effects of taxes commonly discussed. Tim Besley and Torsten Persson, two pioneers of this literature, argue that it has been recognized that "[...] the power to tax is about much more than raising tax revenues – it is at the core of state development" (Besley and Persson (2014), page 100).

At the heart of this literature is the concept of fiscal capacity – coined by historical sociologist Charles Tilly (Tilly, 1975, 1990) – which is usually understood to represent the level of development of a country's fiscal system. Often, it is also considered as a more general indicator of state capacity and, in particular, of the ability of a government to implement complex policies (Rogers and Weller, 2013). From a theoretical perspective, Besley and Persson (2010) show that investments in legal and fiscal capacity are often complements. This leads to strong links between the ability to raise taxes and to provide market-supporting institutions. The complementarity between fiscal capacity, state de-

velopment and institutional quality means that rich countries are also high tax countries, with good enforcement of contracts and property rights.[6] Dincecco and Prado (2012) and Dincecco and Katz (2016) offer convincing empirical evidence of the positive effect of fiscal capacity on long-term economic performance at country level, whereas Dittmar and Meisenzahl (2016) offer evidence of this effect at city-level for pre-modern Germany.

The relationship between fiscal capacity and wider institutional quality is multifaceted. Fiscal capacity furnishes the necessary resources for the provision of public goods and the creation and maintenance of a qualified and efficient bureaucracy able to monitor the economy and society and to implement complex policies. The provision of public goods, such as public education, can also foster the creation of upper-tail human capital and help attract highly skilled individuals through immigration (Dittmar and Meisenzahl, 2016). Even in the absence of large scale public goods provision, state capacity can foster growth and market integration by protecting from external predation, removing institutional barriers to trade, limiting the ability of local elites to extract rents, and offering widely applicable rule of law and regulations (Epstein, 2000). A more effective buereacracy might also be better able to resist the vested interests and rent seeking of the elites, and to raise taxes in a less distortionary way (Koyama and Johnson, 2017). A high fiscal capacity, particularly when accompanied by effective restraints on the executive, is also indicative of an at least partly successful and functional bargain between the state and the economic elites. A successful bargain with the more or less broad elites is particularly important in the historical context because of the quasi-voluntary nature of taxation which still prevailed in some countries in the interwar period, for example Switzerland (Farquet, 2012). Apart from facilitating the taxing of a non-negligibile share of resources, elite cooperation can also promote the implementation and success of policies.

The measurement of fiscal capacity is directly connected to these considerations. The share of taxes in GDP, the share of income (direct) taxes in GDP and the share of income (direct) taxes in total tax revenues are all widely used indicators of fiscal capacity. In my research, I also rely on these. The key insight is that the amount and types of resources the state is able of to tax are both important elements of fiscal capacity. They exemplify the two aspects of institutional quality outlined above: 1) the amount resources available to support the state's infrastructure, 2) the level of cooperation of the elites. Income (direct) taxes are a particularly good indicator of both. Income taxes are generally considered to be some of the most demanding to collect in terms of monitoring and fiscal infrastructure.

[6]The incidence of taxes in GDP in rich countries is around 40%, while the same figure for developing countries is around 10-20% (Besley and Persson, 2014).

Moreover, reaching a consensus between the state and the more or less broad elites is a fundamental prerequisite for effective direct taxation. At the other end of the spectrum are trade taxes. These are considered easy to collect, the tax base – goods entering and/or leaving the country – is easy to monitor, and the political bargain with the elites more straightforward. Indeed, Sokoloff and Zolt (2007), amongst others, note that richer countries have tax systems that are more progressive and rely on personal and corporate income taxes and broad-based property taxes, whereas poorer ones rely mainly on taxes on consumption, excise taxes and custom duties.

2.1.2 Limits to the development of fiscal capacity

If one takes a long historical view, some of the changes in fiscal systems may seem abrupt. However, the difficulties in creating centralized revenue raising institutions are severe and have undermined the process of state formation for long stretches of human history, with the first *modern* fiscal states appearing in Europe only in the last 2-300 or so years. Moreover, centralized taxation represents a necessary, but insufficient condition for the creation of effective states. Parliamentary supremacy – i.e. an effective control of the executive – is generally considered to be a necessary complementary feature to fiscal centralization. This became widespread only in the 19th century in Europe (Dincecco, 2015).

Tilly (1975) provides an overview of the distinguishing features that made Western Europe as a whole particularly prone to the development of the first modern states. One is a certain degree of cultural homogeneity due to the diffusion of the Roman Empire, Christianity, long-standing trade links and similar family structures, despite the existence of different languages, ethnicities and customs. Tilly argues that these facilitated "[...] the diffusion of organizational models, [...] the expansion of states into new territories, [...] the transfer of populations from one state to another, and [...] the movement of administrative personel from one government to the other." The Roman Empire also mattered, according to the author, because its remnants provided the justification for the existence of a sovereign power and for the consolidation of power within the context of relatively similar political institutions. A large literature has explored these and other factors to explain the "rise of the West"; see Koyama and Johnson (2017) for a recent overview.

A consensus in the literature is that reforms of fiscal constitutions are often the result of extreme circumstances (O'Brien, 2011). Wars and conflicts, such as the French Revolution, the English Civil War and the two World Wars, are examples of the type of events that

can have a major impact on the role of governments in the economy and on the way taxes are raised. Tilly (1975) famously argued that "War made the state and the state made war".

Finding direct empirical evidence for this link is, however, not straightforward. Aidt and Jensen (2009), for example, find no indication that war spurred the introduction of personal income taxes in Europe, North America, Oceania and Japan in the period 1815-1939, but their evidence is broadly consistent with expenditure pressure increasing the likelihood of the introduction of such taxes. Sabaté (2016) does find a link between warfare and fiscal expansion by analyzing the period between the mid 19th century and the present, but also highlights that this is U-shaped in technological changes, with nuclear weapons determining the downward sloping part of the curve. He also finds that political institutions can influence the persistence of fiscal reforms brought about by changes in the nature of warfare. Dincecco, Fenske, and Onorato (2016) also find that warfare spurred the development of fiscal capacity. However, they highlight that in the African continent it also led to the creation of special-interest states and the perpetuation of conflict. In Europe, instead, it facilitated a decrease in civil strife and the rise of general interest states.

Queralt (2017) recently argued that these mixed results and the differential impact of warfare on state formation is at least partially explained by the mix of instruments - taxation and borrowing - countries used to finance wars. In particular, in periods in which loans were more widely available due to conditions in financial markets, for example after the development of a global capital market in the 19th century, warfare was not associated with significant fiscal development due to the possibility of defaulting on the loans after the end of the conflict. In any case, in parts of the world characterized by a relatively low incidence of large scale conflicts and where conflicts took place in different historical circumstances from those of Europe, as was the case in Latin America and Asia, warfare alone cannot explain the development of states and the variation of fiscal capacity across space and time (Centeno, 1997, 2002; Gupta, Ma, and Roy, 2016).

Major macroeconomic events can also lead to fiscal reforms by, for example, increasing the demand for the provision of public goods such as unemployment insurance. The Great Depression in the United States is an example of this at both the federal and state level (Wallis and Weingast, 2005; Gillitzer, 2017). In general, any exogenous increase in the demand for public goods can have both static and dynamic effects on the accumulation of fiscal and other state capacities. However, differing degrees political resistance to increases

in taxation will eventually determine how much the state is able to actually expand its franchise (O'Brien, 2011; Hoffman, 2015).

The underpinnings of the bargain between the economic elites, the broader public and the state regarding taxation are disputed in the literature and are very probably contingent on time and place. Resistance to taxation can emerge, for instance, from a desire not to cede resources to an unaccountable and unrestrained sovereign. When a sovereign cannot credibly commit to refrain from confiscation, as is often in the case in absolutist regimes, a low fiscal capacity scenario might be the only viable equilibrium, as shown by Ma and Rubin (2017) for Qing China.

Both political scientists and economists have also argued that resistance to taxation can have its roots in the unwillingness to share resources with categories of people perceived as different. These differences can be due, for example, to racial or regional identities. In comparing the evolution of income taxation in Brazil and South Africa over the course of the 20th century, Lieberman (2003) draws on the concept of the National Political Community (NPC) to argue that institutionalized racial discrimination in South Africa made the white elites more willing to share resources with a group perceived as homogenous to them: poorer whites. In Brazil, on the other hand, the formal rejection of racial discrimination – which was, instead, perpetrated informally – made the white elites more prone to resist the pooling of resources though taxation. The legacy of this early 20th century set-up, according to Lieberman, can be seen today in South Africa's successful income tax and Brazil's continuing struggles. In general, many studies have found a negative relationship between ethnolinguistic fractionalization and the provision of public goods (Alesina, Baqir, and Easterly, 1999; Alesina and La Ferrara, 2005). Recent work, however, suggests that the effect of ethnic and linguistic diversity is not univocal and depends, in practice, on the actual degree of cross-linguistic and cross-ethnic interactions, with more mixing leading to more resource sharing (Desmet, Gomes, and Ortuño-Ortín, 2016).

Religious identity can also play a role in the provision of public goods. Apart from being an important factor in identity politics, it can also have an impact through its traditional role in the provision of services normally associated with public goods, such as health and education. As shown by Chaudhary and Rubin (2016) for the Indian Princely States, the private provision of education by religious organizations of the same creed as the ruler lowered the supply of those public goods to the population as a whole. Public goods unaffected by religious provision did not exhibit this effect.

As argued by Mares and Queralt (2016), however, fiscal reforms need not be connected to interclass, interethnic and inter-religious distributional issues or, more broadly, to the financing of the welfare state. The authors argue that the introduction of the income tax in Britain, for example, was tied to the redistribution of the tax burden within the economic elite, from the traditional land-owning class to the recently emerged industrialists.

Even when starting conditions and shocks provide favorable conditions for big changes in fiscal systems, these cannot happen overnight due to the learning process and the investments in fiscal and human capital necessary for levying new taxes. An illustrative example of this is the taxation of the opium trade in Iran, the development of which stretched over two decades of experimentation in the first half of the 20th century (Hansen, 2001). Indeed, long-run empirical studies confirm a high level of persistence in fiscal systems; see for example Sokoloff and Zolt (2007) on the Americas.

Thus, countries with inefficiently weak states unable to raise sufficient revenues and to provide growth and welfare enhancing public goods have existed for most of history and continue to exist today in many parts of the world. This is the central message of much of the literature on this topic, which, in one form of the other, maintains that history matters in the creation and persistence of institutions.[7]

2.1.3 Fiscal capacity, borrowing and macroeconomic policy

The underdevelopment of fiscal capacity does not only limit countries' long term growth potential, but also their maneuvering space to deal with cyclical fluctuations in economic activity and shocks more generally. As Besley and Persson (2010) point out, fiscal capacity is not fully utlized at all times, but past investments make it possible to raise revenue when this is necessary. An example of this is provided by Sylla and Wallis (1998) who highlight the role of pre-existing state-level revenue structures in the debt crisis of the 1840s in the US. The authors argue that some states faced insurmountable obstacles to raising additional taxes, leading to a wave of defaults following the 1839 recession.[8] Another is provided by O'Brien (2011) who highlights that Britain historically faced less constraints to the expansion of its fiscal base than its main rival powers France, Spain, Austria, Denmark, Russia and the Ottoman Empire. This means that it was able to weather fiscal

[7] See, amongst many others, Acemoglu (2005); Acemoglu, Ticchi, and Vidigni (2011); Besley and Persson (2009); Besley, Ilzetzki, and Persson (2013); Besley and Persson (2014); Dincecco and Prado (2012); Dincecco (2015); Dincecco and Katz (2016); Hoffman (2015); Koyama and Johnson (2017).

[8] According to the authors, newly established frontier states had narrower tax bases than older ones, and relied on property taxes which were too politically costly to expand rapidly in order to continue servicing the debts.

and financial crises with more ease and with more rapid recoveries.

A key tool to deal with adverse shocks is borrowing, which has historically been intimately linked to fiscal development. Economic historians have long tied Britain's exceptional ability to borrow to its early development as an advanced fiscal state, for example. North and Weingast (1989) argued that the shift of power from the king to parliament after the Glorious Revolution of 1688 opened the way for more extensive taxation by transferring control of the fiscal resources to broad elites. This, in turn, increased Britain's credibility, allowing it to borrow extensively and relatively cheaply on financial markets. Some authors stress the importance of earlier periods in British constitutional and fiscal history, particularly the Commonwealth and Civil War (O'Brien, 2011), but the main message is unchanged.[9] Bordo and White (1991) argue that the superior strength and credibility of its fiscal system allowed the United Kingdom to borrow and inflate its economy more extensively than France during the Napoleonic Wars.[10] In a comparative setting, Dincecco (2009) shows that that European countries with centralized and/or limited regimes – the two preconditions for effective, high capacity states – enjoyed cheaper access to credit in the period 1750-1913.

Theoretical models have also formalized the idea that the efficiency of tax systems endogenously affects creditworthiness and thus the ability to borrow (Aizenman, Gavin, and Hausmann, 2000; Arellano and Bai, 2016; Bi, Shen, and Yang, 2016). Esslinger and Müller (2015) show that the relationship between capacity and borrowing can also go the other way. Through a political economy model that endogenizes choices regarding investment in fiscal capacity, while explicitly allowing for public debt and the possibility of default, the authors show that borrowing can facilitate investment in fiscal capacity, but only if income fluctuations are not too large.

Limits to the ability to borrow are still a key factor in fiscal policy today. Gavin and Perotti (1997) have argued that borrowing constraints have been important determinants of pro-cyclical fiscal policy in Latin America. Mendoza and Oviedo (2006) suggested that the more severe financial frictions faced by developing countries in borrowing markets, combined with greater tax revenue volatility, can explain the procyclicality of their fiscal

[9] Stasavage (2016) paints a less optimistic picture. The author argues that institutional reforms rest on two different narratives that are often conflated. On one side are executive constraints, shared governance and transparency, which are generally perceived as good. On the other side are restrictions on the influence of tax payers and the devolvement of decision power to creditors, which are redistributive in nature and not necessarily welfare enhancing. The author argues that British and European history support the latter narrative more consistently.

[10] O'Brien and Palma (2016) make a similar argument on the monetary side, highlighting the importance of the Bank of England's previous commitment to an orthodox monetary policy.

policies and their lower debt-to-GDP ratios compared to industrial countries.[11] With many countries still having to "graduate" from harmful fiscal policy procyclicality (Frankel, Vegh, and Vuletin, 2013), understanding the origin of the constraints that keep countries locked in this inefficient policy space is an extremely relevant issue.

2.1.4 My contribution to this literature

The papers of this dissertation are closely tied to the themes of this literature. At the same time, they aim to help fill some of the existing gaps in our understanding.

My paper on local fiscal institutions in Brazil explores established themes in the literature: public revenues, public expenditure, and the provision of public goods. However, it does so in an under-explored setting where our knowledge of the historical development of fiscal institutions is still limited (Hoffman, 2015; Koyama and Johnson, 2017). My findings tie this development to other key aspects of Brazil's history – slavery and mass immigration – adding another layer of innovative contribution. Moreover, Brazil is a country where the low quality of institutions still plays an important role today. Gaining an understanding of its origin has a clear policy relevance.

My paper on aggregate fiscal dynamics utilizes the concept of fiscal capacity in a setting where it has largely been ignored. Although the literature clearly suggests strong links between fiscal capacity and borrowing capacity, the relationship between fiscal development and cyclical fiscal policy has not been explored directly. This is particularly true in the Great Depression literature, which has mostly focused on the constraints imposed by the Gold Standard and on the impact of WWI to explain the different experience and policy choices of countries during the Depression. My paper points to the fact that even more fundamental and deeply-rooted factors, such as fiscal development, contributed to the course of the Depression.

Finally, my paper on the Great Depression debt crisis studies one of the ultimate results of fiscal dynamics: sovereign default. Although I do not use the concept of fiscal capacity directly in this paper, I show that the collapse in tax revenues behind the defaults cannot be attributed to explicit policy choices and was, instead, rooted in a deep-seated fiscal fragility.

[11] See Ilzetzki (2011) for an alternative, political economy-based, explanation for fiscal policy procyclicality. This is grounded in disagreements amongst successive governments regarding the distribution of public spending.

2.2 Sovereign defaults: theory and evidence

In the first paper of this dissertation, I study the debt crisis that engulfed central and local governments around the globe in the 1930s. The paper is related to several strands of literature. Firstly, it speaks to the literature on the causes and consequences of the Great Depression defaults. Secondly, it relates to three main topics within the large literature in international finance and macroeconomics dealing with sovereign defaults: 1) the link between fiscal policy and sovereign defaults; 2) the role of asymmetric information in defaults; 3) debt crises prediction. In this section, I mostly discuss recent work in these three areas in conjunction with some more general literature on sovereign defaults. This discussion provides a foundation for the literature on the Great Depression debt crisis discussed in the paper.

2.2.1 Inability to pay and default

Sovereign defaults take place when national governments are either unwilling or unable to repay their debts. In either case, creditors have no or limited ability to recover their loans through courts or the seizure of assets. Moreover, when the debtor is located in another country with respect to the creditor, the status of sovereign defaulter can be extended to sub-national governments and even private debtors, who can be shielded by their country's sovereignty. Distinguishing between inability and unwillingness to repay is often a difficult exercise in practice, as political and economic constraints interact with each other. Nonetheless, the distinction is useful in thinking about the causes and consequences of defaults.

A further distinction can be drawn within defaults due to the inability to repay. Following Arellano and Bai (2016), one can distinguish between *fiscal* and *aggregate* defaults. The former occur when the government is unable to raise tax rates in order to collect the necessary resources for repayment due to limited fiscal and/or state capacity. The latter take place because of an economy-wide resource constraint, which entails that raising tax rates will not be sufficient to allow the debtor to repay its debt.

A fair number of recent papers has focused on improving the definition and measurement of debt sustainability. A share of these aim to identify the adjustments to fiscal policy (essentially the size of the primary surplus) necessary to ensure that the debt ratio does not go beyond the country's fiscal limit (Ghosh, Kim, Mendoza, Ostry, and Qureshi, 2013; D'Erasmo, Mendoza, and Zhang, 2016). Others investigate how taxing efficiency and

exogenous shocks affect fiscal limits (Arellano and Bai, 2016; Bi, Shen, and Yang, 2016). A connected strand of literature focuses on how output volatility and the persistence of shocks, both of which affect fiscal limits, can become evident only in "bad times" due to asymmetric information between creditors and debtors (Catão and Kapur, 2004; Catão, Fostel, and Kapur, 2009; Catão, Fostel, and Ranciére, 2011, Forthcoming).

This literature has emerged, amongst other reasons, to reconcile the fact that most models of default stress the debt burden as one of the most important determinant of default (Eaton and Gersovitz, 1981; Arellano, 2008; Panizza, Sturzenegger, and Zettelmeyer, 2009; Mendoza and Yue, 2012) with the empirical observations that: 1) some countries tend to default with low levels of debt, a phenomenon known as "debt intolerance" (Reinhart, Rogoff, and Savastano, 2003); 2) the link between "bad times" and default is tenuous (Tomz and Wright, 2007, 2013; Durdu, Nunes, and Sapriza, 2013).[12] The recently developed definitions of fiscal sustainability highlight the fact that fiscal limits are country specific and that they can change endogenously following shocks. These adjustments go a long way in harmonizing the theoretical foundations of default with the empirical facts.

2.2.2 Reputation and the costs of default

At least since the work of Grossman and Van Huyck (1985), inability-to-pay defaults have been seen as rational responses to adverse shocks in the presence of asset market incompleteness. This is because these defaults make up for the absence of state contingent debt securities, which pay less or not at all during downturns. Under these circumstances a default should be considered "excusable", and should not lead to any reputational consequences for the defaulter.

However, researchers have shown that defaults do affect the conditions under which countries can access financial markets. Although there was no strong evidence for reputational costs until the recent past (Panizza, Sturzenegger, and Zettelmeyer, 2009; Oost-

[12] Tomz and Wright (2007) analyze the empirical content of the hypothesis that countries will default in "bad times" and find only a weak negative relationship between default and output in a sample of 175 sovereign borrowers from 1820 to 2005. The authors conclude that a difficult economic situation is neither a sufficient nor necessary condition for sovereign default. The authors show that sovereigns defaulted when output was below trend only 60% of the time, and that the average deviation of output from trend at the start of a default was only -1.6%. Calibrated default models, instead, predict default almost exclusively when GDP is below trend and when this deviation is on average -8%. The authors' results are consistent with further research on different time periods, samples of countries and approaches to measuring trends in output (Durdu, Nunes, and Sapriza, 2013). Tomz and Wright offer some suggestions on how to reconcile theory and empirics. Importantly for this paper, they argue that "bad times" might need to be defined differently. Good candidates in signaling impending difficulties in debt repayments are large decreases in exports and government revenues, as well as high world interest rates. Time aggregation might also cloud the results of this type of empirical exercise if default is caused by large albeit short-lived declines in output not captured by annual data (Tomz and Wright, 2013).

erlinck, 2013), Cruces and Trebesch (2013) have shown that larger defaults lead to longer exclusions from capital markets and to higher borrowing costs. Their insight is simple, but powerful: defaults vary greatly in size and duration and so should their consequences. Catão and Mano (Forthcoming) find even stronger reputational costs using different metrics for gauging the default premium, a larger sample and a longer time-span. They also find that once the length of the default is accounted for, the additional informational content of haircut size for the default premium is small.

The existence of reputational costs can be explained by the fact that defaults are rarely exclusively driven by inability to pay. Unwillingness to pay can pay an important role and, in fact, researchers have long investigated why sovereigns would ever willingly choose to repay their debts. The choice presumably hinges on its costs and benefits. Early theoretical contributions stressed the role of direct sanctions – for example in trade – and of future access to financial markets as enforcement mechanisms for sovereign debt. Cole and Kehoe (1998) further elaborated that the reputational consequences of default need not be limited to future access to borrowing, but can lead to the defaulting nation as a whole being seen as untrustworthy in all relationships. Consistently with this notion, Esteves and Jalles (2016) show that, in the first era of globalization (1880-1913), the private sector suffered from very large and long-lived reductions in access to credit following sovereign default.

Research on more recent default episodes also suggests that default can have broad-based negative consequences on the international economic activity of countries not connected to explicit sanctions. Indeed, defaults often precede large decreases in trade and foreign direct investment (Rose, 2005; Fuentes and Saravia, 2010; Martinez and Sandleris, 2011), as well as current account reversals and capital flights (Mendoza and Yue, 2012). Default is also often accompanied by other large negative macroeconomic events, such as financial crises (Reinhart and Rogoff, 2009).

2.2.3 The heterogeneity of defaults

As mentioned, defaults are vastly heterogeneous. In his review of the historical literature, Oosterlinck (2013) highlights the difference between various types of contract breach, with mild defaults – involving only interest payments and possibly sinking funds – on one hand of the spectrum and complete repudiations on the other. The theoretical literature, however, has only recently started catching up with the notion of partial defaults (Arellano, Mateos-Planas, and Rìos-Rull, 2013). Oosterlinck also points out that what might not

be considered default by jurists might be so for creditors. The clearest case of this is the repayment of international debts through the printing and debasing of currency. Naturally this applies only to countries able to borrow in their own currency. The inability to do so has been dubbed "original sin" by Eichengreen and Hausmann (1999).

Defaults can also vary greatly in their ex-post characteristics, like haircut size and duration, which in turn can determine the severity of reputational consequences for the defaulters. Esteves (2013) shows that creditor organizations can play a big role in these outcomes: before 1914, systematic bondholder representation provided by the British-based Corporation of Foreign Bondholders reduced haircut sizes and the length of defaults compared to both unsystematic post-default creditor coordination, which was altogether detrimental, and the intervention of underwriting banks.

Defaults can furthermore be "selective" and involve only certain categories of debts or creditors. Erce (2012) identifies three types of default episodes: 1) neutral, 2) discriminatory against foreign creditors, 3) discriminatory against domestic creditors. This classification is based on a number of indicators such as amounts involved, haircuts and the timing of involvement. Erce finds that the foreign or domestic origin of the "liquidity pressure" – i.e. difficulty in rolling over short-term debts – is crucial in selective defaults. A weak banking sector can also play a role by discouraging domestic default, due to its potential adverse effect on domestic banks. Finally, Erce argues that a stronger reliance on foreign finance for the functioning of the economy would make debtor countries more reluctant to undergo an external default.

Historically, the most common type of discrimination has been against foreign creditors (Reinhart and Rogoff, 2011). However, there are instances of discrimination between different classes of domestic and foreign creditors as well. Eichengreen and Portes (1988) show, for example, that US creditors were treated less favourably than their British counterparts in several cases during the interwar period.[13] The existence of different repayment probabilities for different classes of creditors gives rise to the possibility of an additional enforcement mechanism for sovereign debt. This is the transfer of debt securities to creditors least likely to be defaulted on through transactions on secondary markets. This mechanism can lower or eliminate the incentive to default (Broner, Martin, and Ventura, 2008, 2010).[14]

[13] A prominent example of this is the German default. Political pressure from the UK government and the non-interventionist attitude of the US led to a more favourable settlement for British nationals.

[14] By analyzing the German debt crisis during the Great Depression, Papadia and Schioppa (2016) illustrate the strong impact secondary markets can have on the actions and expectations of both the authorities of the debtor country and the creditors.

2.2.4 Predicting debt crises and defaults

Notwithstanding a large body of literature, the determinants of sovereign defaults are still not well established empirically. Moreover, serious identification issues have forced researchers to concentrate on identifying predictors of default rather than strictly causal factors.

Manasse and Roubini (2009), for example, provide a systematic assessment of factors related to sovereign debt crises for the period 1970-2002 using data mining techniques. They find ten variables to be relevant for predicting sovereign debt crises: the total external debt/GDP ratio; the short-term debt reserves ratio; real GDP growth; the public external debt/ fiscal revenue ratio; CPI inflation; the number of years to the next presidential election; the U.S. treasury bills rate; external financial requirements (current account balance plus short-term debt as a ratio of foreign reserves); the extent of exchange rate overvaluation and exchange rate volatility.

A more recent contribution by Catão and Milesi-Ferretti (2014) concentrates on the role of Net Foreign Liabilities (NFL). The authors highlight that looking at net rather than gross liabilities, particularly net external debt, could be more frutiful in predicting external crises, and that foreign exchange reserves are the most effective mitigating factor for external crises.

2.2.5 My contribution to this literature

My work follows the general methodology of papers attempting to predict sovereign debt crises by studying the association between economic and political indicators and defaults. Contrary to these studies, however, the objective of the paper is not maximizing predictive power, but disentangling the importance of each explanatory variable in the default process. The Great Depression setting allows me to do this thanks to the of the fact that it represents a single huge common negative shock (see the paper for more details).

As it emerges clearly from the discussion above, defaults are complex phenomena and are often part of larger episodes in which many things happen at the same time. This requires the testing of several channels at once to avoid omitted variable bias. Analyzing political or economic factors in isolation will not yield robust predictions, given that it is their joint effect that pushes countries towards default (Manasse and Roubini, 2009). For this reason, I have collected a large amount of new data in order to perform this type of exercise. In the analysis, I include all variables found to be important in previous work for which data is available.

The paper also takes several additional cues from the literature compared to previous studies. In line with the empirical evidence, I construct a new measure of default that allows me to capture partial defaults. In addition, I allow explicitly for the possibility of default having feedback effects on other variables by employing a dynamic structure in my estimations. Finally, I measure "bad times" in different ways. In particular I analyze the impact of output contractions, fiscal crises and trade collapses separately.

The finding that contractions in public revenues turn out to be the most robust and reliable predictors of default, even after controlling for the size of the economic contraction, supports the idea that fiscal limits are not fixed entities. The collapses of tax revenues experienced by some countries reveal that the Depression affected the ability of states to tax and/or revealed new information regarding the fiscal fragility of the defaulting countries. A further interesting possibility is that the size and pervasiveness of the slump might have revealed fragilities that were unknown not only to the creditors, but to the debtor countries themselves.

2.3 Slavery, institutions and development in Brazil

In this section, I provide an overview of the history of slavery in Brazil and on its impact on the development of its institutions and economy. The account of the nature of slavery in Brazil relies heavily on Herbert Klein and Francisco Vidal Luna's excellent and exhaustive 2010 book "Slavery in Brazil" (Klein and Luna, 2010). The review of the impact of slavery draws from a broad literature focused on Brazil and beyond.

2.3.1 The establishment and evolution of slavery

The Atlantic slave trade began almost immediately after the colonization of South America and the Caribbean by Spain and Portugal. By the 1580s, the forced trasportation of people from Africa to the Americas had already reached the considerable size of around 3,000 individuals per year.

The establishment of sugar plantations in the Northeast of current Brazil provided the initial stimulus for the large scale import of African slaves to the country. For various reasons, including the spread of non-endemic diseases and the distatste of the Spanish crown for the enslavement of indigenous people after the unification of Spain and Portugal in 1580, the initial enslavement of local populations was not successful. Pre-existing links between Portugal and slave markets in Africa proved useful in facilitating this new enterprise and meant that the Portuguese were early movers in the exploitation of African

slave labour in their new colonies.

The defining characteristics of the establishment of slavery in Brazil compared to previous slave-based societies were three, according to Klein an Luna: 1) the prevalence of slaves amongst agricultural workers, 2) the production of commodities for international trade, 3) the influence of slaves on local society. Brazilian slavery did not only represent the largest forced transfer of people during the Atlantic slave trade with nearly 5 million individuals, but also provided the blueprint for subsequent slave-based enterprises: the Brazilian model influenced developments in the whole New World and the establishment of colonies by the English, French and Spanish.

Although the Brazilian sugar industry lost its primacy in the world by the end of the 17th century, it continued to play an important role in world supply and in the demand for slave labour. Additionally, the discovery of gold – and later diamonds – in the country's interior (today's Minas Gerais and Goiás) provided new stimuls for the import of slaves to Brazil. This boom would last until the second half of the 18th century. In the same century, cotton started becoming an important export in the coastal province of Maranhão, further stimulating demand for slave labour. Slaves continued to be a fundamental source of labour for the province's cotton plantations until the 1840s (Pereira, 2017). The late 18th century also saw a revival of the sugar industry and an increase in its producion to commercial levels in Rio de Janeiro and, to a lesser extent, São Paulo, which continued into the 19th century. This increased slave numbers in this relatively peripheral area.

The last wave of slave imports took place in independent Brazil and was due to the huge expansion of the coffee industry in the 19th century. This involved the whole Southeastern region of the country. By the 1830s, coffee was the country's most important export and by the 1850s Brazil was the world's largest producer, responsible for over half of total global production (Klein and Luna, 2010). Combined with developments from the previous century, the growth of coffee production led to a sharp increase in the share of slaves entering the country trough Southeastern ports, mainly Rio de Janeiro. By 1820, these had overtaken the Northeast's traditional slave ports (Figure 2.1).

Rio De Janeiro and the Paraíba valley located across the western border of the latter and the eastern border of São Paulo were the earliest centers of coffee mass production. The crop then spread at an uneven pace over the century to Minas Gerais and to the rest of São Paulo, leading to the exploitation of virgin lands in the west of the state. The exploitation of these new territories – still inhabited by independent indigenous people – began in the 1870s and 1880s, as the abolition of slavery was looming large and eventually

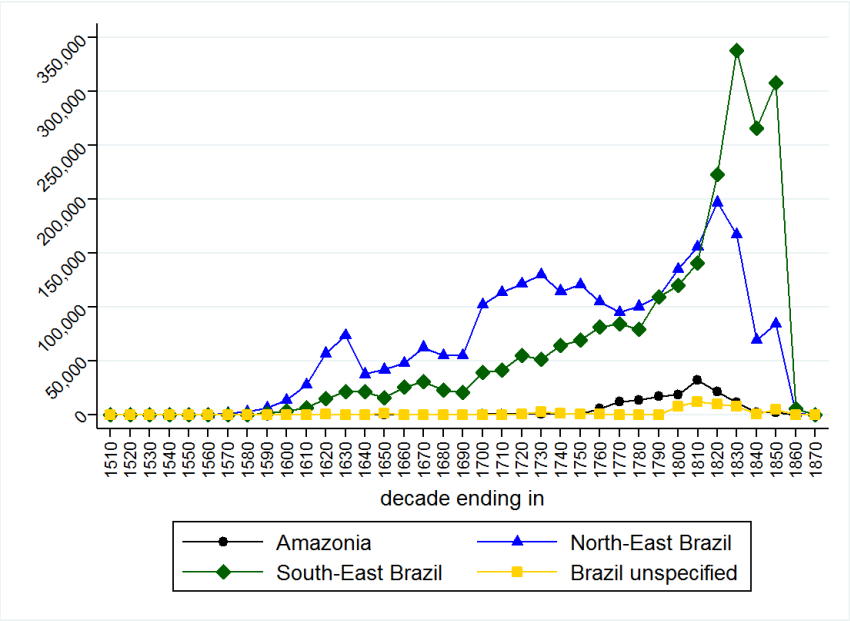


Figure 2.1: Estimate of the number of slaves disembarked in Brazil per decade during the Atlantic slave trade according to area of arrival, 1510-1870

Source: Voyages Database: The Trans Atlantic Slave Trade Database (2009)

became reality. After the end of slavery, the coffee frontier continued to expand westwards and southwards and eventually reached the state of Paraná. Coffee overtook sugar as São Paulo's most important crop in the 1840s and by the end of the decade the province was Brazil's second most important producer after Rio, which was eventually overtaken in the 1880s.

The peripheral role of São Paulo in colonial Brazil meant that its relationship with slavery was markedly different from that of the sugar-producing North-East and even from that of its neigbours Rio de Janeiro and Minas Gerais. The former was home to the country's colonial capital since 1723, engaged in substantial sugar production, and featured a key port important for both the trade in slaves and commodities. As such, Rio de Janeiro received and retained large numbers of African slaves well before the coffee boom of the 19th century. Minas Gerais, instead, was the most important mining centre in the country. The discoveries of gold and diamonds in the 18th century stimulated a large inflow of slaves, but also the development of its vast lands for a variety of agricultural enterprises such as cattle ranching and, later, coffee production.

Up until the mid 18th century, the coercion of labour in São Paulo mainly concerned the indigenous population through the institution known as the *aldeamento* (Summerhill, 2010; Klein and Luna, 2010). These were settlements aimed at tying the semi-nomadic population to the land, converting it to christianity and offering protection from other

indigenous populations. With the start of large scale coffee production in the early 1830s the situation started to change, but São Paulo still only contained 79,000 slaves by the late 1830s compared to 78,000 in 1819. Rio de Janeiro, instead, already had over 146,000 slaves and Minas Gerais nearly 170,000 in 1819. By 1872, Rio de Janeiro still had roughly twice as many the slaves as São Paulo – 306,000 compared to 157,000 – and Minas Gerais almost two and a half times more (Marcílio, 1975). Even after African slavery had picked up in São Paulo it was markedly different from that found other parts of Brazil. Slave holding was generally small scale, evenly distributed geographically and characterized by more balanced sex ratios.

During the 19th century, international pressure against slavery rapidly mounted. British influence was particularly important in the piecemeal abolition of slavery in Brazil. In a 1827 treaty, Britain forced Brazil to commit to the abolition of the slave trade within 3 years from its ratification. The treaty also gave British ships the right to inspect their Brazilian counterparts on the high seas to ascertain the presence of slaves. The Law of 1831, approved during a cull in the slave trade, nominally criminalized the transportation of slaves from Africa to Brazil, in agreement with the treaty. However, it was almost completely ineffective and local juries, controlled by wealthy landowners, absloved the few cases of smuggling of slaves brought to the courts. At this point, the opposition to slavery within Brazil was very low. Moreover, the expansion of coffee production and the very limited slave revolts encouraged the continuation and even an increase in the reliance on coerced labour. Unhappy with the situation, Britain reserved itself the right to treat slave ships like pirate ships and to apprehend and try slavers in British courts with the Bill Aberdeen of 1846. Following this increased pressure, numerous incidents between the British navy and the slavers, and the fear of losing British support in the face of a military threat from Argentina, a series of new provisions were introduced in 1850 under the auspice of the Brazilian Minister of Justice Eusébio de Queiroz. These reinforced the 1831 Law, were effectively enforced, and de facto ended the slave trade. Slave arrivals fell from 54,000 in 1949 to 3,300 in 1851 (Fausto, 1999) and faded out after that.

A remarkable characteristic of Brazilian slavery in both the 18th and 19th century was, besides its scale, the vast variety of activities the slaves were involved in. Apart from plantations, slaves could be found in urban and rural activities such as: fishing, cotton weaving and spinning, ranching, artisanship, food production, domestic services, construction, transportation and industry. However, the vast majority of active slaves recorded in the 1872 census worked in agriculture – 808,000 out of around 1.2 million

– and many of the rest in closely related activities. The number of slaves working in coffee production was approximately 320,000, more than one quarter of total active slaves. Although the majority of slaves did not work on plantations even in the Southeast, sugar and coffee municipalities still had the highest concentration of slaves (and the most unequal distribution thereof).

This pattern became more pronounced with the abolition of the international slave trade, after which the internal trade increased in importance. Between 100 and 200 thousand slaves were transported from the Northeastern and the Southern provinces of Brazil to the Southeastern ports of Rio de Janeiro and Santos benween 1850 and 1888 (Mattoso, 1986). The transfer of slaves also involved individuals within the same province. These transfers usually relocated slaves from declining areas to the booming coffee-growing regions. This intraprovincial trade might have been even more important than the interprovincial one, but is less well recorded (Klein and Luna, 2010).

As a result of these developments, the concentration of slaves on plantations increased at the same time that slave prices shot up and slave numbers diminished. This might be an indication that, as the cost of slaves went up, their viability over free labour was higher where the slaves could be organized in gangs and "incentivized" more effectively with violence to perform tasks that required mostly physical effort and little skill or care. This would be in line with the conceptualization of coerced labour provided by Fenoaltea (1984) and with the experience of the United States and the Caribbean (Klein and Luna, 2010). The former, in particular, also experienced a *Second Middle Passage*, which led to the relocation of close to a million individuals from coastal areas to the cotton and tobacco plantations of the interior in the 19th century (Berlin, 2003).

As the 19th century progressed, Brazil witnessed a rise in abolitionist sentiment and in slave resistance and unrest. Slavery was finally abolished in 1888 (see the paper for more details).

2.3.2 Slavery, inequality and development: the general picture

Stanley Engerman and Kenneth Sokoloff posited that different initial endowments drove the differential use of slave labor in the Americas. This in turn led to different levels of economic inequality, which affected the development of institutions crucial for growth (Engerman and Sokoloff, 1997, 2012). With regard to taxation, Sokoloff and Zolt (2007) argued, along similar lines, that the initial level of inequality determined the different development of fiscal institutions in various parts of the Americas. They suggested that

Latin America's high level of initial inequality explains its particularly deficient fiscal capacity, even in comparison to countries with similar levels of income. According to the authors, low levels of direct wealth and business taxation – due to low local level taxation – are a key factor in the radically different patterns of taxation between North and Latin America, which can still be observed today.[15]

These assertions have been contested from a conceptual and empirical perspective. North, Summerhill, and Weingast (2000) contended that factor endowments, at least in the long-run, are endogenous. The authors furthermore stressed the importance of political institutions in the late 18th and 19th centuries in shaping successive outcomes. Coatsworth (2005) argued along similar lines. By comparing Latin American coffee exporters, Nugent and Robinson (2010) point out that endowments alone cannot explain the different economic and political evolutions of these countries. In line with North et al, the authors indicate that different political institutions played an important role in the differential impact of coffee production on economic and political outcomes. More broadly, Williamson (2010) maintained that Latin America did not have high inequality in comparative terms during the colonial period and up to the mid-19th century. Therefore, he argued, inequality is a product of more recent times, and a central tenet of Engerman and Sokoloff's argument does not stand up to scrutiny.

Engerman and Sokoloff's hypothesis has also found only partial direct empirical confirmation. Nunn (2008b) tests it at country level for the Americas and across states and counties in the US, finding a negative effect of slavery on economic development. However, he finds no support for the hypothesis that this effect was driven by large scale plantation agriculture or that the channel of persistence was inequality. In their sub-national study of the effect of different types of colonial activities in the Americas, Bruhn and Gallego (2012) find evidence that activities characterized by increasing economies of scale are related to lower GDP per capita today, but find only weak evidence of a relation between the latter and forced labour. Moreover, they do not find evidence of these colonial activities being linked to income inequality. They do highlight, however, that political inequality might have played an important role.

Evidence on the detrimental effect of slavery and other forms of coerced labour on various aspects of development in the Americas, if not in Brazil, is abundant. Apart from

[15] For Spanish America, Irigoin (2016) also identifies the legacy of colonialism as one of the main determinants of state weakness and low fiscal capacity in the continent. However, she stresses different aspects of Spanish rule compared to Engerman, Sokoloff and Zolt, namely the disconnect between those who collected and those who spent public revenues and the weak political representation of the citizens in the colonies.

the already mentioned work by Nunn (2008b), Bertocchi and Dimico (2014) highlight the persistently negative impact of slavery on income inequality in the US through the channel of differential educational provisional and attainment between blacks and whites. Acemoglu, García-Jimeno, and Robinson (2012) illustrate the negative impact of slavery on long-term development in Colombia, while Dell (2010) shows that coerced labour in mining in Peru (the *mita*) is associated with worse economic outcomes today. Interestingly, in her story, districts where the institution was absent had higher historical land inequality and this might have actually helped the provision of public goods and the protection of property rights. Once again, the evidence points to more complex and ambivalent mechanisms than those highlighted by Engerman and Sokoloff.

Evidence of the adverse developmental impact of coerced labour exists outside of the Americas as well. Buggle and Nafziger (2016), for example find that the incidence of serfdom in Russia in 1861 is associated with lower well-being today, while Markevich and Zhuravskaya (2015) show that its abolition led to a rapid increase in living standards and industrial development. The Atlantic and other slave trades also left deep scars on the sending regions of Africa. Nathan Nunn and co-authors have illustrated this in a series of papers (Nunn, 2008a; Nunn and Wantchekon, 2011; Nunn and Puga, 2012).

2.3.3 The legacy of slavery in Brazil

Criticisms of Engerman and Sokoloff's hypothesis of a direct line from initial endowments to economic outcomes today via colonization have traction for Brazil, as well. The deep structural changes underwent by the country since its Independence suggest a much more complex story. Nonetheless, the legacy of history on Brazilian development can be clearly identified.

Naritomi, Soares, and Assunção (2012) study the impact of two extractive colonial commodity booms – sugar and gold – on long term development in Brazilian municipalities. They find that areas nearer to the booms are associated with worse economic outcomes today, such as land concentration and public goods provision. The authors also find similar effects to those of sugar and gold for the early coffee boom of the 19th century, but no effect for the late 19th early 20th century boom of the same commodity, suggesting that the exploitation of natural resources under less extractive institutions need not have adverse effects on long-term development. The authors also find that distance from Portugal, a proxy for colonial control, interacted with the extractive booms and determined the extent of their adverse effects on development. Naritomi et al, however, do not unpack the

channels through which these effects occurred or persisted through time. The differential use of slave labour and its institutional legacy are plausible, but unexplored candidates.

The 18th century colonial diamond boom in Minas Gerais analyzed by de Carvalho (2015) provides further evidence of the complex relationship between endowments and development. The author finds that the extraction of this commodity also had a long-run impact, but of the opposite sign compared to gold and sugar. De Carvalho links the positive effects of historical diamond production to better historical infrastructure provision and a relatively low exploitation of slave labour.

Fujiwara, Laudares, and Valencia Caceido (2017) offer the only clear-cut direct evidence of the legacy of slavery within Brazil. The authors exploit the Tordesillas line that divided Portuguese and Spanish holdings in South America as a discontinuity for the intensity of slavery across Brazilian municipalities. The authors find that municipalities with more slaves in 1872 are characterized by higher income inequality today, as well as stronger racial imbalances in income and education and worse public institutions.

In an attempt to uncover the roots of regional inequality in Brazil, Musacchio, Martínez Fritscher, and Viarengo (2014) analyze a more recent period. The authors suggest that export growth during the Old Republic (1889-1930) had strongly positive effects on the provision of public education in Brazilian states . They also find that this effect was muted in states that had more slavery before abolition or cultivated cotton. However, the authors make no attempt to establish a causal impact of slavery.

With their longitudinal approach, Musacchio et al convincingly show that cross-sectional correlations between historical circumstances and outcomes today ignore the deep structural changes underwent by Brazil over time, as evidenced by dramatic changes in the educational ranking of Brazilian states between 1872 and 1940.[16] However, while the authors emphasize the Republican period as one of deep transformation, which shaped the face of modern Brazil, structural changes were well under way already in the monarchic era. In many ways, the developments of the Republican period were simple continuations of trends that had started in previous decades. The expansion of railways, the decline of slavery, the start of mass immigration and the coffee boom, all of which crucially contributed to the rise of the Southeast of the country, are clear evidence of this. Indeed understanding structural changes within Brazil is in good part understanding the rise of the Southeast, and of the province/state of São Paulo in particular, from the mid 19th century onwards.

[16] Martínez Fritscher and Musacchio (2010) and Martínez Fritscher (2011) provide further arguments and analysis along these lines.

Summerhill (2010) offers a comprehensive illustration of the long-term effect of historical institutions in São Paulo. Despite being a relative backwater in the colonial period, the province had its own form of coerced labour: the already-discussed *aldeamentos*. These were settlements featuring both "extractive" and "inclusive" characteristics. With the beginning of the coffee boom, more systematic and large-scale forms of labour coercion became salient in order to satisfy the rapidly growing labour demand. African slavery proliferated in the province and, in fact, was substantially more widespread in the monarchic period than in the colonial era. The snapshot captured by 1872 census, which I use in my analysis, likely represented the peak of slavery in the province, at least in terms of absolute numbers.

The author finds no relationship between slavery in 1872 and long term development, as measured by income per capita in 2000.[17] He moreover finds and a positive relationship between *aldeamentos* and development. Additionally, the author finds no relationship between long term development and the classical channels proposed in the literature for the existence and persistent effect of extractive institutions: historical inequality and political enfranchisement. The author ascribes this result to the fact that, following the structural change and reversal of fortunes of the 19th century, slavery in 1872 measured production possibilities and thus future prosperity rather than the incidence of an extractive colonial institution. However, the IV strategy he employs to deal with these issues does not yield any conclusive evidence either. When he performs a cross-state study with earlier levels of slavery – those in 1819, although the quality of this data is uncertain, as Summerhill points out – he finds the expected negative result.

Funari (2017) performs a similar analysis and extends it to additional Brazilian states – Minas Gerais, Pernambuco and Rio Grande do Sul – confirming the lack of a significant relationship between political inequality and long-term economic development and finding mixed results regarding the link between economic inequality and development, presumably due to the different colonial experiences of these states.[18] Reis (2017) attempts to link municipal growth between 1920 and 2000 for the whole of Brazil to a series of factors including wealth concentration in 1920, the extent of the political franchise in 1914 and the incidence of slavery in 1872, finding no robust relationships. The authors finds, instead, a positive effect of the foreign population share in 1920. No attempts to estimate causal relationship are made by these authors, but the results are once again illustrative of the

[17] To be precise, the author initially finds a positive association between the incidence of slavery and income per capita, which, however, disappears with the inclusion of further controls.

[18] The main difference between Funari's and Summerhill's studies is that the latter constructs his inequality measures for 1905 whereas the latter uses the 1920 census.

absence of a clear relationship between slavery and development. What emerges, however, is a potential link between slavery, migrant settlement and development, which I explore in my work.

de Carvalho Filho and Colistete (2010) focus on a different aspect of development in São Paulo by analyzing the historical determinants of the provision public education. They find that the presence of foreign born immigrants positively affected the supply of public education. They argue that the channel for this effect was the establishment of community schools for the different immigrant groups, the cost for which was partly covered by public funds. The authors further link the presence and positive impact of foreign farm laborers and foreign farm owners to the coffee boom. However, the authors do not find a robust relationship between slavery, or land inequality, and the supply of public education today. My findings suggest that this might be due to the fact that slavery at least partially shaped the settlement of foreign immigrants, and thus the outcomes studied by the de Carvalho Filho and Colistete.

2.3.4 My contribution to this literature

In summary, although a number of studies have attempted to investigate the impact of slavery or have suggested links between their findings and the incidence of forced labour, the institutional and economic legacy of slavery in Brazil has not been satisfactorily established. Despite a large volume of scholarship on the origin and evolution of slavery in Brazil, with Klein and Luna (2010) representing one of the most recent examples, the legacy of a monumental event like the Atlantic slave trade is still nowhere near being fully fleshed out in the largest slave importer of the time. My third paper offers a contribution in this direction by focusing on a key factor for long run development: fiscal institutions.

The paper also offers a new perspective on slavery in Brazil by highlighting its impact on the settlement of foreign migrants during the age of mass migration. Between 1880 and 1909 well over 2 million migrants entered Brazil, and most of them were absorbed by the plantations and growing factories of the Southeast of Brazil. Although migration is widely recognized to have decisively contributed to the the country's economic and political development, its relationship with slavery has not been explored. I argue that slavery influenced the settlement of migrants both directly and indirectly. On one side, large shares of slaves in the population reduced the demand for free labour. On the other side, the presence of large scale slavery and its legacy after abolition discouraged the settlement of migrants due to the extremely negative connotation the institution had acquired by the

second half of the 19th century. The geographical mobility of the immigrants and their ability to punch above their weight in terms of political influence due to Brazil's severe labour needs provided the ingredients for the improvement of local public institutions and the provision of public goods. The combination of these factors helped to shape the development of local institutions in Brazil and, potentially, its long-term growth trajectory.

Chapter 3

Paper 1: Sovereign Defaults during the Great Depression: The Role of Fiscal Fragility

3.1 Introduction

The main contribution of this paper is to revisit the pivotal sovereign debt crisis of the 1930s by introducing substantial innovations on the data and methodological side compared to previous studies, and to focus on a so-far largely unexplored factor: fiscal fragility. The paper also offers a different perspective and approach compared to the existing literature on sovereign defaults by focusing on a single debt crisis and its dynamics. I show that the deterioration in public revenues that accompanied the Great Depression was a key predictor of the wave of defaults of the early 1930s. This result emerges clearly both from a simple visual analysis of the data and from rigorous testing. Moreover, I identify the role of public revenues strongly and separately from the direct impact of the Depression gauged by measures of macroeconomic health, such as changes in GDP and trade. The finding is furthermore robust to the inclusion of a wide array of controls commonly used in the literature and to the use of different estimators.

Although the workhorse model of default, which has evolved from the seminal work of Eaton and Gersovitz (1981), posits that sovereigns will renege on their obligations in "bad times" if they cannot issue state-contingent debt securities, the empirical support for this hypothesis is mixed (Tomz and Wright, 2007, 2013). My finding that, during the Great Depression, the trajectory of tax revenues had predictive power beyond that of other macroeconomic indicators suggests that the response of countries to economic shocks can

be vastly different and that "bad times" might be captured more effectively by looking at public revenues rather than GDP, as suggested in recent work (Tomz and Wright, 2007; Arellano and Bai, 2016). It furthermore indicates that this variable contains information about the path of future fiscal policy – and thus the sustainability of the debt – and/or about structural characteristics of countries that affect their ability to respond to shocks i.e. their "fiscal fragility".

Collapses in public revenues in the 1930s were not connected to more expansionary and intentionally unsustainable fiscal policies. On the contrary, the countries which saw their public revenues fall drastically after the onset of the Depression in 1929 were also more likely to experience large decreases in expenditures and increases in their primary surpluses, as well as defaults. Thus, it appears that defaulters attempted to direct their policy toward debt sustainability through austerity, but could not achieve the desired result. Non-defaulters, instead, were able to run relatively more – for this conservative, pre-Keynesian era – lax fiscal policies. The result also holds for sub-national governments, which experienced severe contractions in their revenues as well.

The more likely explanation for the connection between revenue loss and default is that defaulters were inherently more fragile than non-defaulters, and that the onset of the Great Depression revealed the full extent of their vulnerability. In other words, structural factors – such as their fiscal and administrative capacity – shaped countries' response to the Great Depression shock and the probability of default. In my second paper, (Chapter 4)I show that, indeed, country characteristics – summarized by fiscal capacity indicators – were crucial in determining the response of countries to the Depression: more fiscally capable countries were able to avert the worst of the collapse in public revenues and to continue tapping credit markets.

The 1930s offer a rich environment in which to study sovereign defaults. After a large, but short-lived lending boom in the second half of the 1920s, the onset of the Great Depression in 1929 kick-started the (arguably) largest debt crisis in history. Between 1931 and 1936, many national and sub-national governments around the globe interrupted interest and principal payments on their foreign loans. Following Reinhart and Rogoff (2013)'s definition of external default [19], almost 45% of countries in their sample of 70 countries were in default in the first half of the 1930s.

Although there is substantial research on the widespread domestic and international

[19]The failure to meet an interest or principal payment on the due date or within the specified grace period. The episodes also include instances where rescheduled debt is ultimately extinguished in terms of less favourable than the original obligations.

consequences of the 1930s defaults, there is surprisingly scant research on their causes. The most common narrative around the time of the crisis was that the misguided and excessive international lending of the 1920s triggered the defaults (Harris, 1935; Madden, Nadler, and Sullivan, 1937; Lewis, 1938; Lary, 1943). More recent research has highlighted the role of "bad luck" in crisis (Diaz-Alejandro, 1983; Fishlow, 1986) and its systemic nature (Kaminsky and Vega-García, 2016; Accominotti and Eichengreen, 2016). Additionally, Flandreau, Gaillard, and Panizza (2010) have argued that the distortions in international financial markets were not as pervasive as previously thought, strengthening the "bad luck" interpretation. A partial challenge to the "bad luck" view is the work of Eichengreen and Portes (1986), who find that both exogenous economic factors and political choices played a role in the defaults.

Compared to previous work, I provide a wealth of new information, which allows me to test systematically for the first time a range of channels that have been proposed in the literature on sovereign defaults in general and on the Great Depression debt crisis in particular. Most importantly, I provide a more complete and sophisticated dataset on public debts than available until now (Reinhart and Rogoff, 2009; Abbas, Belhocine, El Ganainy, and Horton, 2010) – which accounts for maturity structures and sub-national borrowing for almost 30 countries worldwide – and a new measure of default size.[20] On the methodological side, I employ panel data methods, which offer clear advantages compared to previous studies of the 1930s defaults.

This paper also offers a different perspective compared to studies looking at many default episodes over time. The objective of these is usually to maximise their ability to predict sovereign crises and defaults (Manasse and Roubini, 2009; Catão and Milesi-Ferretti, 2014). For this reason, they attempt to identify powerful and parsimonious predictors of default, often by combing different variables. Examples of these are public revenues over external debts, reserves over external debts or net foreign assets. While these variables improve predictive ability, they also make it impossible to disentangle the contribution of each of their components. The goal of this paper, instead, is to identify the role of each of these macroeconomic indicators in anticipating default. From an econometric perspective, I am able to single out the predictive ability of all of my variables because I can trace their evolution over the course of a single large event, the Great Depression, which crucially

[20] At the national level, the sample includes Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, the Netherlands, Norway, Poland, Romania, Sweden, Switzerland, the United Kingdom, Uruguay and Venezuela; at the local level data is unvaiable on a consistent basis for Austria, Chile, Czechoslovakia, Greece, Romania and Venezuela, but is available for Yugoslavia.

also spawned a sufficient number of defaults for a meaningful analysis.

Analyzing a short time span, presents a further advantage over studies aggregating many default episodes over long stretches of history because the latter may be unable to adequately control for the changing international context in which the defaults took place. This context can affect both the probability of default and the explanatory variables used to predict external crises. Naturally, studying a single default wave presents drawbacks, as well as advantages, from both an econometric and conceptual point of view. The main ones are a relatively small sample size and potential difficulties in generalizing the results to other episodes of sovereign debt crises i.e. external validity.

In any case, the findings of the paper raise a number of interesting points related to the literature on shocks, fiscal policy and default, particularly recent work attempting to better define and measure fiscal limits (the maximum sustainable debt-to-GDP ratio of a country). Ghosh, Kim, Mendoza, Ostry, and Qureshi (2013) and D'Erasmo, Mendoza, and Zhang (2016), for example, focus on the conditions needed to achieve fiscal solvency i.e. adjustments in the primary surplus following changes in the debt level. Bi, Shen, and Yang (2016) and Arellano and Bai (2016) consider fiscal limits driven by the efficiency with which the government is able to tax, which bounds effective tax rates. Bi, Shen, and Yang (2016) further consider how fiscal limits can change endogenously following, for example, real exchange rate shocks. Finally, Catão, Fostel, and Ranciére (2011, Forthcoming) concentrate on the role of asymmetric information, suggesting that creditors only receive information about the true equilibrium path followed by a debtor country, and thus its fiscal limit, in "bad times". I review this literature alongside the most relevant part of the large empirical and theoretical literature on sovereign debt and default in the introductory chapters of this dissertation (Section 2.2).

This paper cannot provide definitive evidence for all these mechanisms. However the results do support an interpretation of the Great Depression defaults, which is consistent with a number of them. The idea that countries defaulted simply because they hit some fixed fiscal limit is ruled out by the results. While I do find that countries were more likely to default if they had a larger debt burden, this factor is neither the only nor the strongest predictor of default. Instead, the fact that public revenues collapsed more pronouncedly than output in some countries and that these countries were more likely to default suggests that the Great Depression endogenously lowered fiscal limits by affecting states' ability to raise revenues, revealing the true fragility of countries' fiscal systems.[21] Therefore, the

[21]The extent to which the governments of debtor countries were aware of their own fragility is unclear in an era of very imperfect data collection and in the face of a huge shock like the Great Depression.

findings suggest the need for a convergence between the literature on taxing-efficiency-driven fiscal limits, the literature on state and fiscal capacity (see Besley and Persson (2014) for an introduction), and the literature on asymmetric information. This is also warranted by more recent default crises (1970-2011), which were also accompanied by a reduction in tax collection as a share of GDP (Catão, Fostel, and Ranciére, 2011).

Besides the role of fiscal fragility, several further results emerge from the analysis. Contrary to suggestions in the historiography and in the theoretical economics literature, I find no relationship between external defaults and the maturity structure of public debts at the national-provincial level. For municipal defaults, instead, the liquidity pressure associated with a high share of short term debts is linked to default size. I rationalize this finding with the fact that scarce resources for repayment were allocated hierarchically between different levels of government. For national-provincial defaults, I find that the size of the public debt burden anticipated the occurrence of defaults, consistently with most models of sovereign default. A greater reliance on the external sector of the economy in terms of both trade and finance, instead, is negatively associated with national-provincial defaults, in line with the predictions of reputational models. None of these variables, however, is as robustly associated with default as public revenue deterioration.

The "bad luck" versus "political opportunism" debate on the Great Depression defaults introduced above boils down to two fundamental questions. First, were the defaults the result of misjudgment on the part of creditors and opportunistic behaviour by borrowers or the inevitable result of factors beyond the control of borrowing countries? Second, to what extent were the factors leading to default global in nature or specific to individual countries? Recasting my results in terms of this debate, it is evident that a univocal answer does not do justice to the magnitude and complexity of the 1930s debt crisis. A deep economic and fiscal crisis, which hit countries with different intensities, interacted with political choices, particularly when governments were forced to give preference to either domestically or externally oriented sectors of the economy. Thus, Eichengreen and Portes' conclusion that both politics and economics mattered is confirmed, although I find the mechanisms that led countries to default to be substantially different from those identified by the two authors.

The rest of the paper is structured as follows. Section 3.2 offers a historical overview of the lending boom and debt crisis of the interwar period. Section 3.3 provides details on the newly assembled data-set. Section 3.4 presents the empirical strategy and discusses

Nonetheless, it is reasonable to assume that they had more information than the creditors.

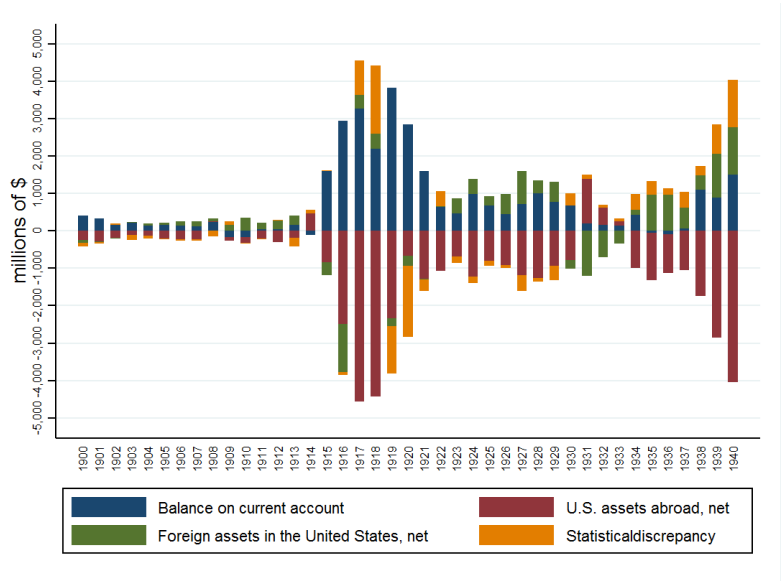
the results. Section 3.5 concludes.

3.2 Setting the stage: borrowing, lending and defaulting in the interwar period

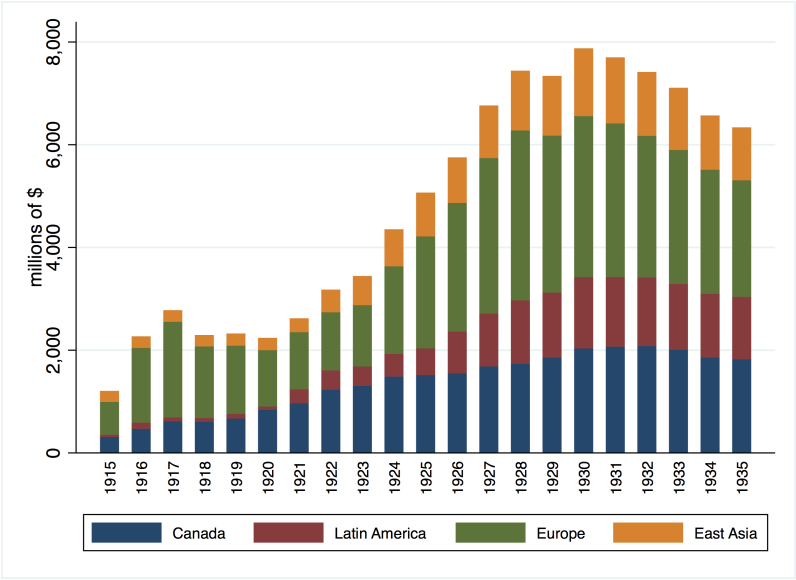
The lift of the ban on foreign branching for US banks embedded in the Federal Reserve Act of 1913 provided a decisive boost for the boom in post-WWI international lending. US banks set up branches abroad to gather intelligence in order to start underwriting and selling foreign bonds (Eichengreen, 1989) and between 1924 and 1931, around 60% of total international lending came from the US (Crafts and Fearon, 2013). By 1929 the dollar had overtaken sterling as the leading international currency in international finance (Chitu, Eichengreen, and Mehl, 2014). Thus, in this period the US underwent a transformation from an essentially closed economy to a an international hegemon (Figure 3.1). The US's predominance, combined with the challenge of finding detailed disaggregated information on loans in other currencies in this period, informs the choice of focusing on defaults on dollar-denominated debts in the analysis below. The fact that US creditors were treated less favorably than their British counterparts at the default stage might induce some non-random measurement error in my default measure, but this differential treatment regarded the post-default stage rather than the choice to default itself and is thus not problematic.

Cycles of international lending and default were hardly a new phenomenon at the time of the Great Depression, but never had the scale of defaults been so large and their incidence so widespread (Winkler, 1933; Eichengreen, 1991). Up to this day, such rampant insolvency is unique, with the potential exception of World War II and its direct aftermath. In the US, although international lending was a relatively small share of all capital issues in the interwar period, default on foreign bonds represented the largest bond default item of the first half of the 1930s (Table 3.1).

Most of the interwar defaults took place in the early 1930s, the exception being a few episodes in the early post-war years (e.g. Brazil, Mexico). The temporal concentration of these episodes motivated the search for a common cause. Contemporary commentators identified the supposedly speculative and misguided international lending of the 1920s as the ultimate cause of the defaults (Harris, 1935; Madden, Nadler, and Sullivan, 1937; Lewis, 1938; Lary, 1943). Their narrative is one of little or no discrimination between good and bad borrowers by the creditors and of the sudden realisation of the unsoundness



(a) US balance of payments, 1900-1940



(b) Net outstanding US loans by region, 1915-35

Figure 3.1: The internationalization of the US Economy

Source: the BoP data is from Edelstein (2006) and the data on US loans is from Lewis (1938).

of investments compounded by the Great Depression shock (Eichengreen, 1991).[22] Recent research supports the idea of a common root for the defaults by emphasizing the systemic nature of the Great Depression debt crisis (Kaminsky and Vega-García, 2016) and the fact that conditions in creditor countries were decisive in triggering the huge sudden stop, which set-off the crisis (Accominotti and Eichengreen, 2016).

[22] Feinstein and Watson (1995) document John Maynard Keynes' doubts on whether American lending to Europe in the 1920s followed the same patterns and principles of UK lending during the Classical Gold Standard era. The underlying conviction was that both lenders and borrowers were driven by distorted incentives, partial or false information, or downright irrationality.

Table 3.1: Annual bond defaults: principal amounts in thousands of dollars, 1930-1935

Year	Railroads	Industrial	Public Utilities	Real Estate	**Foreign**
1930	841	134,994	96,344	128,158	**708**
1931	213,228	443,560	201,722	556,908	**632,015**
1932	201,739	699,034	593,136	543,579	**581,385**
1933	1,087,909	482,228	363,933	416,052	**1,104,748**
1934	310,251	206,435	150,244	83,266	**256,601**
1935	761,701	92,275	149,128	46,785	**9,064**
Total	2,575,669	2,058,526	1,554,507	1,744,848	**2,584,521**

Source: Standard Statistics Co., Standard Bond Investments, Weekly Advisory Section, Jan 11, 1936 p. 2,913 as reported by Madden, Nadler, and Sullivan (1937). As pointed out by these authors, the data in this table has several limitations: it does not represent all bond defaults, but only those large enough to be known by securtiy markets and the compilers of the data.

Table 3.2: Outcome of US lending by geographical area in thousands of dollars, 1920-1935

	Latin America	Europe	East Asia	Total
Total bond investment	1,935,612	3,380.625	869,783	6,186,091
Interest received	692,822	1,708,100	479,523	2,880,445
Principal repayments	693,189,00	1,485,946	395,876	2,575,011
Market value of outstanding bonds	491,108	1,449,007	564,985	2,505,200
Balance	-58,493	1,262,428	570,601	1,774,536

Source: Madden, Nadler, and Sullivan (1937), Table 24 page 147.

Some of the claims of those that saw the crisis as the result of mistakes, distorted incentives and outright dishonesty have also been substantiated. Investigations by the US Congress revealed the unorthodox practices of some brokers and bankers in placing the loans, demonstrating that some degree of malpractice was clearly present (Flandreau, Gaillard, and Panizza, 2010). With regard to German borrowing, for example, Ritschl (2012a) argues that perverse incentives due to the Dawes Plan of 1924 contributed to creating moral hazard on both the borrower and lender side by making reparations junior with respect to commercial debts, leading to excessive borrowing. This set up was then reversed by the Young Plan of 1930, which contributed to a sudden stop and to plunging Germany into economic chaos.

However, the overall picture is not as dire as contemporaries made it to be and default, while pervasive, did not affect all debtors. There is ample evidence of discrimination at the lending stage (Eichengreen, 1989; Eichengreen and Portes, 1990) and subsequent satisfactory rates of return for foreign creditors (Madden, Nadler, and Sullivan, 1937; Eichengreen and Portes, 1988; Jorgensen and Sachs, 1988); see also Tables 3.2 and 3.3. In

Table 3.3: Rate of return on US foreign bond investments by geographical area, 1920-1935

Year	Latin America	Europe	East Asia
1920		7.67	
1921	7.38	7.77	7.07
1922	7.64	7.79	6.39
1923	6.79	7.41	6.35
1924	6.97	7.67	6.64
1925	6.9	7.66	6.45
1926	7.01	7.54	6.48
1927	7	7.3	6.33
1928	7.34	7.5	6.15
1929	6.71	7.44	6.15
1930	6.23	7.44	6.36
1931	4.5	6.52	5.86
1932	1.98	5.47	5.68
1933	1.34	4.27	6.02
1934	1.14	4.91	19.20
1935	1.78	3.93	6.09
Average	5.41	6.77	7.15

Source: Madden, Nadler, and Sullivan (1937), Table 29 page 157.

a comprehensive study of bond issues in the 1920s, Eichengreen and Portes (1988) show that ex-ante yield spreads over risk free domestic options – treasury bills in the US and consols in the UK – more than compensated British investors and almost compensated US investors for the losses of default. By studying all New York bond issues of the 1920s, Flandreau, Gaillard, and Panizza (2010) conclude that the desire to maintain their good reputation meant that underwriters carefully screened and selected loans, leading to less malfunctioning in the international financial markets than previously thought.

Nonetheless, defaults did impact creditors. Jorgensen and Sachs (1988) find large differences in rate of returns between Latin American countries: continuously serviced Argentinean loans yielded higher returns than US Treasury bills, while default in Bolivia, Chile, Colombia and Peru translated into losses for foreign investors. In general, the profitability of single bond issues depended strongly on when they were issued. Those issued during the early 1920s enjoyed unbroken service for a number of years. Later issues only provided remuneration for a a limited amount of time before default hit in the early 1930s.

Defaults also had large aggregate impact on creditor economies. The German default,

for example, contributed to the diffusion and severity of the slump in the US through financial transmission channels (Ritschl and Sarferaz, 2014) and to the UK's departure from the Gold Standard through the exposure of banks (Accominotti, 2012). For these channels, the distribution and timing of losses from defaults, rather than their cumulative amounts, are of central importance. For debtors, the consequences of default were also non-trivial. Eichengreen and Portes (1990) find that defaulting countries recovered faster from the Depression on average, even after controlling for the severity of the slump. Dealing specifically with the case of Germany, Ritschl (2002b) argues that the default was instrumental in the country's recovery in the 1930s.

The impact of the defaults went well beyond the 1930s, however. In establishing the Bretton Woods system, policy-makers – following a classical trilemma framework – decided to forgo the free movement of capital, rather than either fixed exchange rates or an independent monetary policy. At the time, capital flows were perceived as speculative and disruptive in light of the Great Depression experience (Obstfeld and Taylor, 1998). In the US, the debt crisis was furthermore seen – in conjunction with the broader crisis that affected vast swathes of the banking sector – as the result of the failure of banks to manage their conflicts of interests, and was used as a key justification for the division of investment and commercial banking embedded in the 1933 Glass-Steagall Act (Carosso, 1970; White, 1986; Benston, 1990).

Systematic research on the factors that led individual countries to default or not is not abundant. Diaz-Alejandro (1983) and Fishlow (1986) singled out the magnitude of the external economic shock of the Great Depression as the main contributor to default. Eichengreen and Portes (1986), instead, found that both economic "bad luck" factors and political choices mattered. The authors showed that terms of trade deterioration and the foreign debt-to-income ratio were related to the extent of default, but so was the degree to which countries reduced the budget deficit following the onset of the Great Depression, which was at least partially the result of political choices and constraints.[23]

In my analysis, I provide a systematic reassessment of the factors associated with default using new data and econometric techniques. The findings paint a rich picture of the Great Depression debt crisis, in which structural factors, shocks and political choices all played a role. Eichengreen and Portes' general conclusion is thus validated, but, as I show below, not its specific findings. The clearest difference between my results and theirs

[23]The authors also find that their estimates over-predicted the extent of default in Australia, and they linked this to the importance of international political and economic relations, in particular the close ties of the country to its main creditor: the United Kingdom.

is the role of fiscal policy: while they find that austerity generally payed off in terms of avoiding default, I highlight that such attempts tended to fail due to sharp falls in revenue and that, on the contrary, non-defaulters ran relatively more lax fiscal policies.

3.3 Data and descriptive statistics

I have transcribed a large amount of new data from historical sources to perform a comprehensive analysis of the interwar debt crisis. I discuss the data and sources briefly directly below, and in detail in Appendix 3.B.

3.3.1 A new public debt dataset

The principal data contribution of this work is a new public debt data set. This includes both domestic and foreign debts and accounts for their maturity structure. Its main novelty, however, consists in including local public debts. Local level data was left out of previous work presumably due to its very scattered nature. To the best of my knowledge, I am the first to provide information on sub-national public debt on a systematic basis for a sample of almost 30 countries for the interwar period.

The countries included are Argentina, Australia, Austria, Belgium, Bolivia, Brazil, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Estonia, Finland, France, Germany, Hungary, Ireland, Italy, Japan, the Netherlands, Norway, Peru, Poland, Romania, Sweden, Switzerland, the United Kingdom, Venezuela and Yugoslavia. These countries make up over 90% of US net outstanding foreign loans for the whole period examined. Not all time series are available for all countries (see Appendix 3.B), therefore some of the countries drop out in some of the specifications in the econometric analysis (see Section 3.4). In particular, sub-national debt figures are not available on a continuous basis for Austria, Bolivia, Chile, Czechoslovakia, Estonia, France, Greece, Peru Romania and Venezuela.

Reinhart and Rogoff (2009) and Abbas, Belhocine, El Ganainy, and Horton (2010) represent two recent efforts to reconstruct public debt statistics over the very long run. For the interwar period, they both rely on data collected by the League of Nations and later included in a United Nations volume (United Nations, 1948), which is also the starting point of my work. The information in the volume is limited to central government and central government guaranteed debts[24], and breaks these down into domestic – long-term

[24]Government guaranteed debts, normally constituted a small share of total public debts, a notable exception being Australia, where the commonwealth guaranteed all the outstanding debts of the states on

and short-term – and foreign debts. Less complete data on the debt service are also reported.

The compilers of the volume were very transparent about the limitations of their data, which are mainly due to different reporting standards across countries and which pose a serious challenge to international comparability. Appendix 3.B provides details of these issues on a country by country basis. Whenever this was possible, I included or excluded certain items to enhance the comparability of the data, but the overall picture is still imperfect. On the positive side, however, the different reporting techniques reflect the evaluation of the public debt burden by the statistical offices and, presumably, the governments of the debtor countries. As such, they contain valuable information. In order to further mitigate the problem of imperfect comparability, I use panel data methods in the analysis. These rely on the time series variation of variables rather than on the cross-sectional comparison of levels (see Section 3.4). In this respect, my paper has a similar approach to Schlularick and Taylor (2012), who face commensurate issues of cross-country comparability in their study of credit booms and busts.

The principal sources for the local public debt data are the Yearbooks of the German Imperial Statistical Office (Statistisches Reichsamt, 1936b, 1938, 1939/40). For some countries (e.g. Argentina), I integrated these sources with the publications of various bodies, such as the Institute for International Finance – established by Bankers Association of America in cooperation with New York University in response to the interwar debt crisis – the Corporation of Foreign Bondholders – created by private British holders of foreign government securities in 1886 to protect their interests – and rating agency Moody's investment manuals (for details, see Appendix 3.B).

These data reveal that the compilers of the UN volume were not fully aware of the size of local level borrowing. Their claim that central government debt represented the lion's share of public debt is unsubstantiated for a number of countries. Moreover, local public debts, in general, constituted a far from negligible share of total public debts: 31% on average between 1927 and 1936. The relative importance of local debts varied greatly across countries. Figure 3.2 illustrates this point. Nations with federal structures and/or large and independent cities were characterised by major borrowing at the local level. Examples of this type of countries are Brazil, with an average share of local debt over total debt of 71.7% between 1928 and 1934, and Germany, with an average of 49.7% between 1927 and 1936. In more centralised and less sizable countries, local borrowing

1 July 1929.

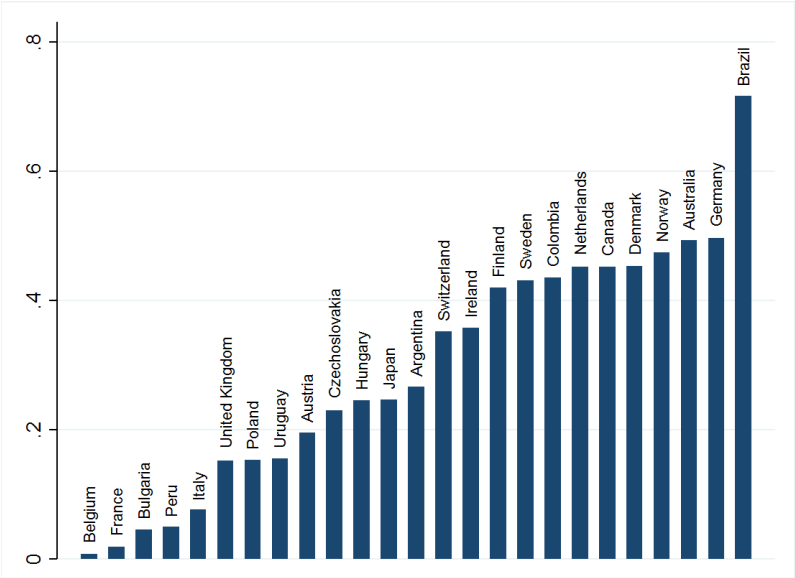


Figure 3.2: Average share of local debt over total debt, 1927-1936

Source: author's estimates based on data in United Nations (1948), Moody's (1931, 1934, 1935), Institute of International Finance (1927), Corporation of Foreign Bondholders (1929), Statistisches Reichsamt (1936b, 1939/40), Werhahn (1937), Francese and Pace (2008). See Appendix 3.B for details.

was much smaller. Belgium and Bulgaria, for example, had an average local share of debt of 0.8% and 4.5% between 1927 and 1936 respectively.

The local debt data indicates that the comparative debt burden picture is seriously distorted by the exclusion of sub-national public debts. Whether national, provincial or municipal, governments service their debts though taxes and other public revenues. In the case of foreign currency denominated liabilities, the servicing of the debt further relies on the availability of foreign exchange. Both public revenues and foreign exchange are generated by the productive activities of the economy; thus, central and local governments rely on the same base to produce the resources needed to meet their obligations. A holistic picture of public debts is thus essential in order to obtain a precise measure of the burden faced by countries, and evaluate with confidence its contribution to default.

3.3.2 A new default size measure

Another important data contribution of this paper is constructing a new measure of default size for the interwar period. This measure is the outcome variable of the econometric analysis of Section 3.4 below. Table 3.4 and 3.5 illustrate my estimates of National-Provincial and Municipal default size. Unfortunately, no distinction can be drawn at this stage between national and provincial defaults due to incomplete disaggregated annual information on outstanding dollar debts for all countries.

Table 3.4: National-provincial default size, 1930-1936

Year	AUT	BGR	CZE	GER	HUN	POL	ROU	YUG	ARG	BOL	BRA	CHL	COL	PER	URY
1930	0	0	0	0	0	0	0	0	0	0	0.03	0	0	0	0
1931	0	0	0	0	0	0	0	0	0	1	0.4	0.78	0	1	0
1932	0.24	1	0	0	0.62	0	0	1	0.02	1	1	1	0.53	1	1
1933	0.28	1	0	0.39	1	0	1	1	0.2	1	1	1	1	1	1
1934	0.34	1	0	1	1	0	1	1	0.26	1	1	1	1	1	1
1935	0	1	0	1	0	0	1	1	0.28	1	1	1	1	1	1
1936	0	1	0	1	0	1	1	1	0.25	1	1	1	1	1	1

Default size is measured as the share of the principal of dollar bonds in default. Source: Authors calculations. Data on outstanding loans is from Lewis (1938) and data on the defaults is from Moody's (1933, 1934, 1935, 1936, 1937), see Appendix 3.B for details. AUT= Austria, BGR=Bulgaria, CZE= Czechoslovakia, GER=Germany, HUN= Hungary, POL=Poland, ROU= Romania, ARG=Argentina, BOL= Bolivia, BRA=Brazil , CHL= Chile, COL=Colombia, PER= Peru, URY=Uruguay.

Table 3.5: Municipal default size, 1930-1936

Year	AUT	BGR	CZE	GER	HUN	POL	ROU	YUG	ARG	BOL	BRA	CHL	COL	PER	URY
1930	0	0	0	0	0	0	–	–	0	–	0	0	0	0	0
1931	0	0	0	0	0	0	–	–	0	–	0.5	0.14	0.46	0	0
1932	1	0	0	0	1	0	–	–	0.6	–	0.83	1	1	1	1
1933	1	0	0	1	1	0	–	–	0.67	–	0.75	1	1	1	1
1934	1	0	0	1	1	0	–	–	0.82	–	0.77	1	1	1	1
1935	0	0	0.24	1	1	0	–	–	0.6	–	0.72	1	1	1	1
1936	0	0	0.23	1	1	0	–	–	0.82	–	0.72	1	1	1	1

Default size is measured as the share of the principal of dollar bonds in default. Source: Authors calculations. Source: Data on outstanding loans is from Lewis (1938) and data on the defaults is from Moody's (1933, 1934, 1935, 1936, 1937), see Appendix 3.B for details.. AUT= Austria, BGR=Bulgaria, CZE= Czechoslovakia, GER=Germany, HUN= Hungary, POL=Poland, ROU= Romania, ARG=Argentina, BOL= Bolivia, BRA=Brazil , CHL= Chile, COL=Colombia, PER= Peru, URY=Uruguay.

The modern definition of default employed by Standard and Poor's, which is used in studies investigating modern debt crises, is a unilateral interruption of the repayment of the principal and/or interest of contractual debt obligations or – an essentially irrelevant case in the interwar context – the offer of a swap of new for existing debt with less favorable terms than the original issue before an interruption of payment, which is accepted by the creditor (Catão and Mano, Forthcoming). The definition used by my source – Moody's investment manuals from the interwar period– is very similar.

I measure default size as the principal of dollar bonds in default compared to the principal of all outstanding dollar bonds. I argue that this ratio represents the best measure of default size at the time of the actual defaults. Given the uncertain and drawn out nature of post-default renegotiations, particularly acute in the interwar period, using ex-post haircuts would be inadequate. These are the outcome of negotiations that take place under economic and political conditions that are potentially unrelated or very distant

from those of countries at the time of their default. Furthermore, they are a static measure and do not reflect the dynamic process of default that is, instead, evident in my default size measure. This measure is also preferable to binary default/no default indicators commonly used in the literature. As discussed above, default episodes are vastly heterogeneous in size, as well as in duration. Binary measures ignore this heterogeneity and discard a large amount of informations that, instead, a continuous default size measure will capture.

My default size measure demonstrates the extent of this heterogeneity in the interwar period. Partial defaults were common at both the national-provincial and municipal levels and often preceded complete defaults. Some defaults remained partial as in the case of Austria, Argentina and Czechoslovakia. In Brazil, although the central and most local governments defaulted, some municipalities – most notably the city of Porto Alegre – continued to service part of their debts. In Poland and Bulgaria, municipal governments did not follow the central government into default at all. As the econometric analysis demonstrates, national-provincial and municipal defaults only share some common factors, while others help to predict one type of default but not the other. Fiscal fragility, however, was a common feature in both types of default.

3.3.3 Some descriptive statistics

Keeping in mind the data issues described above, which limit the reliability of raw comparisons across countries, it is nonetheless useful to explore some characteristics of the newly assembled data. In particular, I will illustrate how these data fit with arguments made in both the theoretical literature and the historiography.

A prominent argument in the historiography is that the maturity structure of international capital flows played an important role in the interwar debt crisis. Feinstein and Watson (1995), among many others, emphasize the unusually large size of short-term flows of the 1920s. Moreover, Germany and South American countries – the most prominent defaulters of the interwar era – relied heavily on short-term borrowing (Jorgensen and Sachs, 1988; Ritschl, 2012a, 2013). Theoretical and empirical papers focusing on more recent crises have also stressed the link between the maturity of debts and the probability of default (Detragiache and Spilimbergo, 2001).

Figure 3.3 demonstrates that the share of short-term debts in domestic public debts debt was indeed generally higher amongst defaulters than amongst non-defaulters. However, after 1924 – when international lending from the US started to take off – differences

between defaulter and non-defaulters were small.[25] Unfortunately, no systematic information is available on the maturity structure of foreign debts. A reasonable assumption is that this was similar to that to that of domestic borrowing. If anything, short-term debts were probably lower as a proportion of external borrowing than of domestic borrowing, given that domestic sources of short-term funds – particularly central banks – were ample, whereas most foreign loans took place by floating bonds. The econometric analysis below indicates that differences between defaulters and non-defaulters were insufficient to help to predict national-provincial defaults. For municipal defaults, instead, I find that the share of short term public debts was a reliable predictor of default. As discussed below, this might be due to the fact that national and provincial debts were prioritized over municipal liabilities when foreign exchange and gold reserves were insufficient to service both.

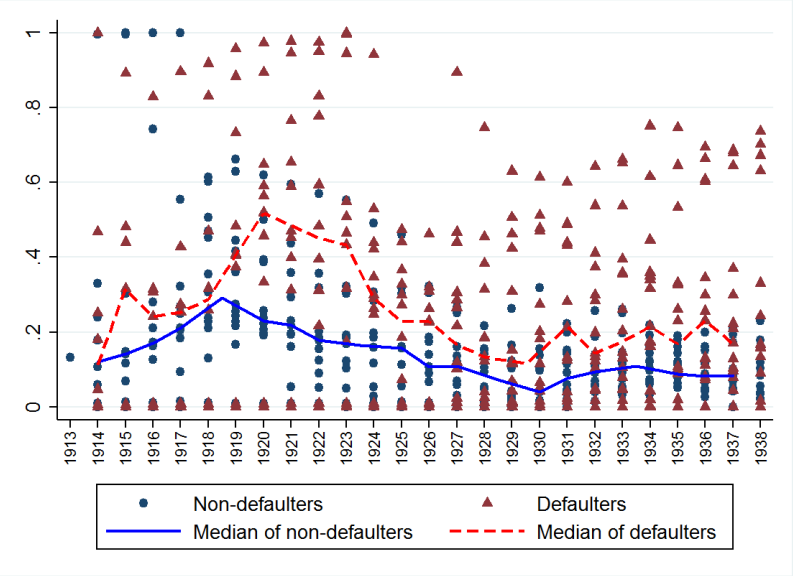


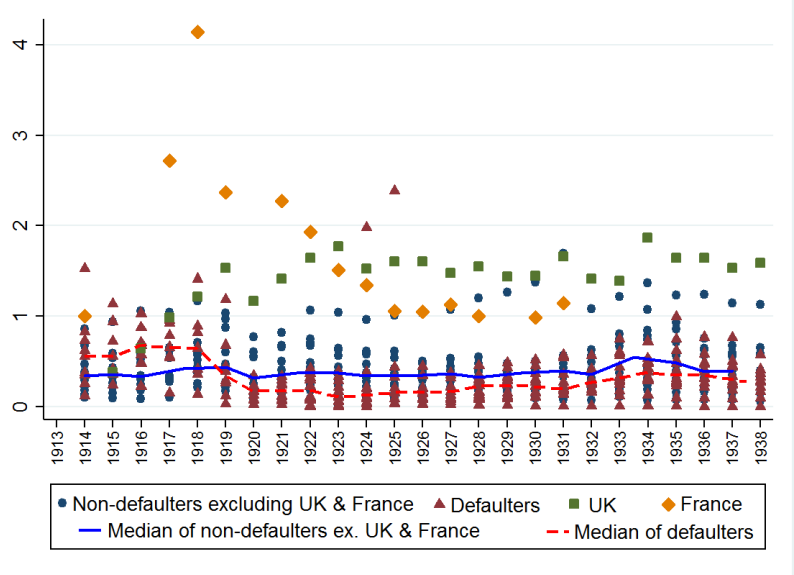
Figure 3.3: Short-term debt as a share of central government debt, 1913-1938

Source: author's calculations based on data in United Nations (1948). Short-term debts are those with maturity of up to two years. Data on the maturity structure of debts is available for to domestic debts only. See Appendix 3.B for details

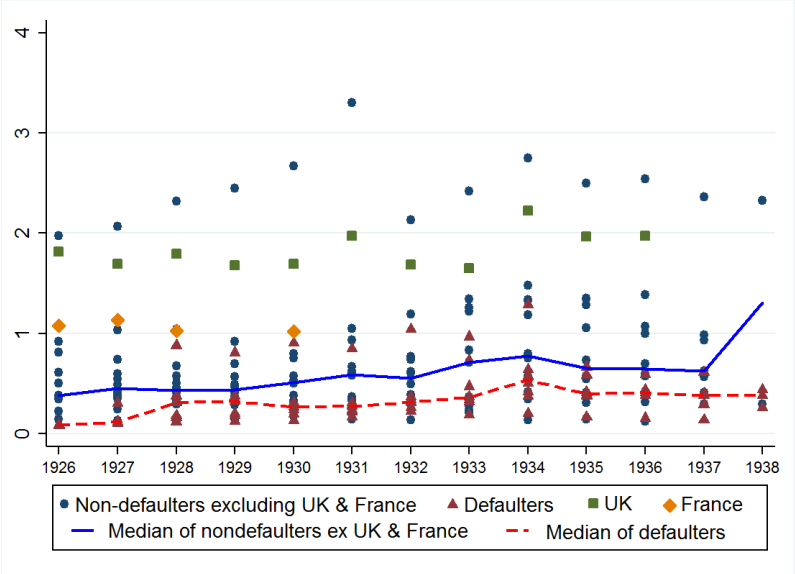
Another important feature of the newly-assembled data is that no relationship is evident between the size of the public debt burden – whether this is just the central government debt, or it includes the borrowing of local authorities – and default (Figure 3.4). Unsurprisingly, given their heavy involvement in WWI, France and the UK emerge as clear outliers with huge debt burdens. That these countries did not default attests to the tenuous unconditional relationship that exists between debt burdens and default. How-

[25] The high incidence of short-term borrowing in the immediate post-WWI period in countries that would later default is extremely interesting in its own right, and could be a further sign of their inherent fragility. Exploring this hypothesis is left for future research.

ever, even excluding their clearly special case, it is still impossible to find a relationship between debt size and default. If anything, non-defaulters had larger debts compared to the size of their economy than defaulters.



(a) Central government debt over GDP, 1914-38



(b) Total government debt over of GDP, 1926-38

Figure 3.4: Public debt burdens and default

Source: author's calculations based on data from United Nations (1948), Moody's (1931, 1934, 1935), Institute of International Finance (1927), Corporation of Foreign Bondholders (1929), Statistisches Reichsamt (1936b, 1939/40),Werhahn (1937), Francese and Pace (2008) for public debts and Klasing and Milionis (2014) for nominal non-PPP adjusted GDP. See Appendix 3.B for details.

While this fact apparently clashes with most models of default, which highlight the debt burden as one of the most important determinant of default (Eaton and Gersovitz, 1981; Arellano, 2008; Panizza, Sturzenegger, and Zettelmeyer, 2009; Mendoza and Yue, 2012), it

is line with a substantial body of literature highlighting the fact that some countries tend to default with low levels of debt. This "debt intolerance" (Reinhart, Rogoff, and Savastano, 2003) has various non-mutually-exclusive explanations. One are differences in a wide range of economic and political characteristics of countries – such as the public's tolerance for austerity and the growth prospects of the economy – and in features of the public debts – like their maturity structure and legal code of issuance (Bowdler and Esteves, 2013). Another is the volatility of countries' macroeconomic fundamentals and the persistence of the shocks their are subject to (Catão and Kapur, 2004; Catão, Fostel, and Kapur, 2009). Finally, past credit history can play a role; for example, by determining how bond yields will react to new information. My analysis finds that accounting for structural characteristics, which affect both the ability to borrow and the probability of default, reestablishes the size of the public debt as an important factor in sovereign defaults also in the interwar setting.

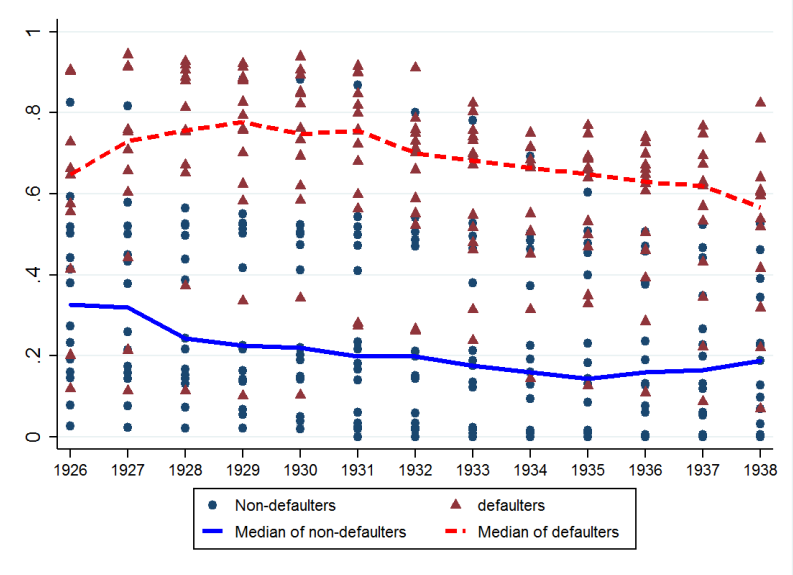


Figure 3.5: Foreign debt as a share of central government debt, 1926-1938

Source: author's calculations based on data in United Nations (1948). See Appendix 3.B for details

What emerges clearly from the raw data is that defaulters relied disproportionately more on foreign borrowing compared to non-defaulters (Figure 3.5). The difference is very significant. For defaulters the median share of foreign debts as a share of central government debt hovered between 60 and 75% in 1926-38 and reached its peak in 1929; it then declined during the 1930s, as international capital flows came to a grinding halt and domestic sources of finance gained importance. For non-defaulters, the median foreign share of debts was approximately 33% in 1926 and declined steadily to 14% in 1935. However, the econometric analysis reverses the result of this raw comparison. Once country charac-

teristics are controlled for, the relationship between default and foreign borrowing turns negative. The result implies that reliance on external sources of finance made countries more reluctant to renege on their foreign payments and face the possibility of being cut off from international financial markets.

The final piece of evidence in this section regards the relationship between default and public revenues. Figure 3.6 shows that defaulters saw their central government tax revenues contract significantly more than non-defaulters. The picture for local governments is very similar. Whereas the defaulters experienced a median contraction of more than 20% in their tax revenues between 1929 and 1933, non-defaulters' public revenues contracted by less than 7%. The analysis below reveals that, even after controlling for a wide array of factors, the relationship between fiscal contraction and default remains statistically significant and quantitatively important.

As outlined above, a deterioration of fiscal revenues can be an indicator of bad economic times. However, it is not necessarily simply the reflection of a slump. The reaction of tax revenues to a fall in income can vary strongly across countries. One reason is that fiscal policy can be different in different countries. Some governments might react strongly by cutting taxes and/or raising expenditures, while others might remain passive or even raise tax revenues in an attempt to continue servicing debts. Another reason is that factors such as the structure of the economy and fiscal institutions can influence the reaction of tax revenues to economic shocks. In my second paper (Chapter 4), I show that the level of development of the fiscal system, as measured by fiscal capacity, strongly influenced the ability of countries to prevent their government financing from collapsing during the Depression.

Defaulters do not appear to have reduced tax revenues within the context of a lax fiscal policy. On the contrary, as Figure 3.7 shows, public expenditure for defaulters fell together with tax revenues after 1930. The primary surplus of defaulters actually improved for a while after 1929, as they attempted to keep servicing their debts and reestablish debt sustainability through austerity. This pursuit failed as the Great Depression progressed, leading to default. The opposite was true of non-defaulters, who were able to run relatively more lax fiscal policies after 1929, and actually expanded their public expenditure consistently after the onset of the Depression. Thus, the evidence indicates that some countries – endowed with more resilient fiscal institutions – were able to avoid collapses in their fiscal aggregates, contractionary fiscal policies and default.

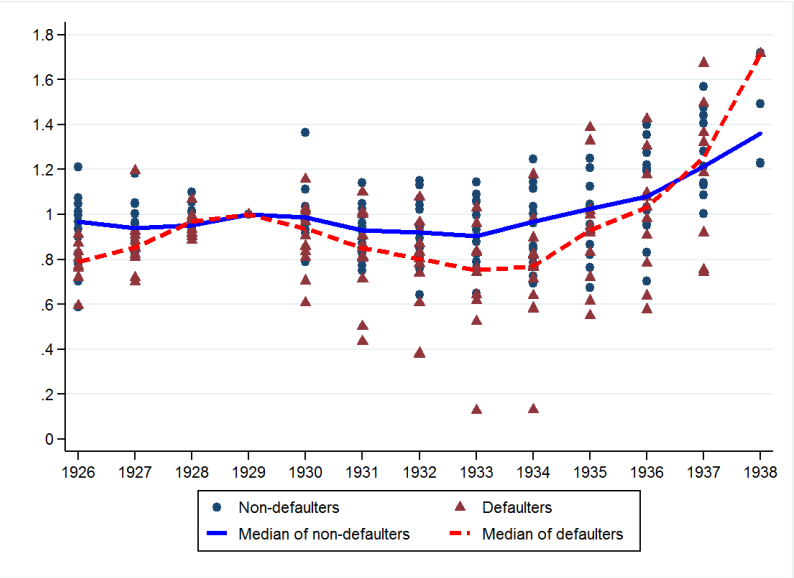


Figure 3.6: Central government tax revenues 1914-1938

1929=1. Source: author's calculations. See Appendix 4.B for details on the sources.

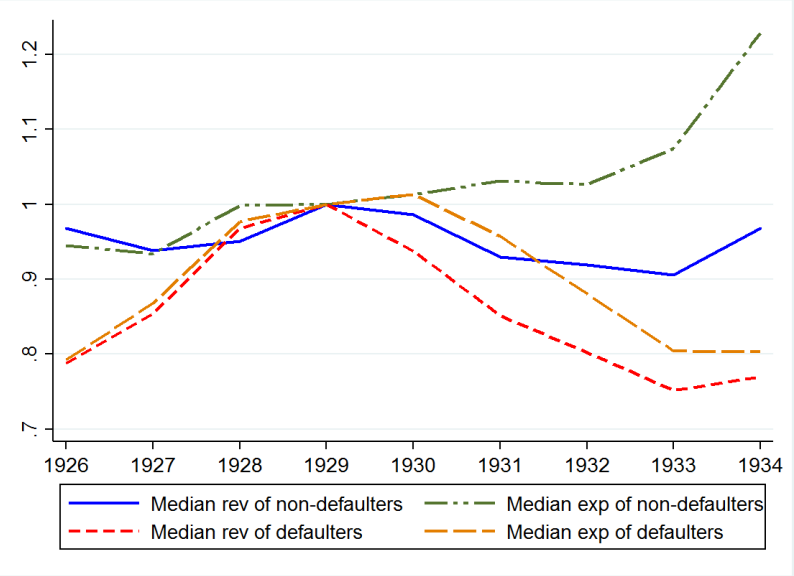
3.4 Econometric analysis

I conduct my analysis on the predictors of default size separately at the national-provincial and municipal levels. I first outline the methodology in detail (Section 3.4.1). Then, I present the findings (Section 3.4.2). In Section 3.4.3 I illustrate some robustness checks, which I then expand in Appendix 3.A. There, I also replicate the analysis of Eichengreen and Portes (1986), which still represents the reference paper for cross-country studies of default during the Great Depression.

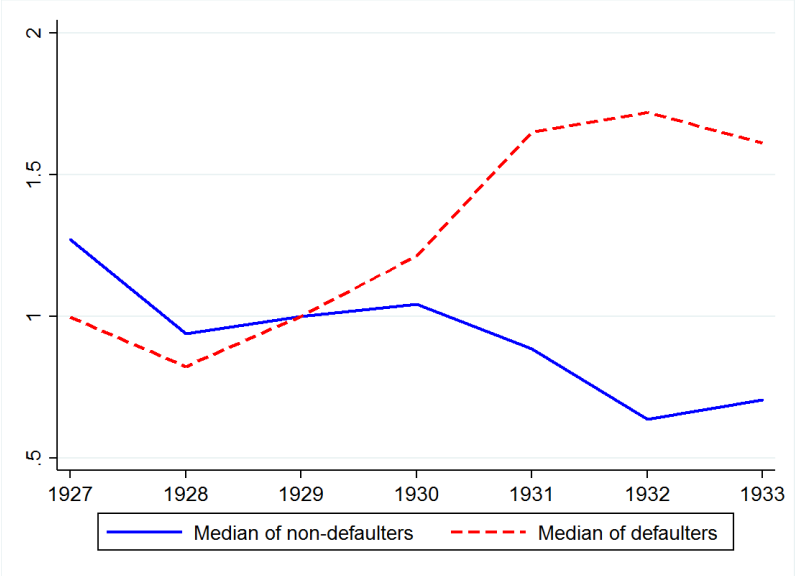
3.4.1 Methodology

Although the primary focus of the analysis is gauging the predictive ability of public revenue deterioration for default, the complex and multifaceted nature of defaults means that I have to account for a variety of factors concomitantly in order to avoid omitted variable bias. As Manasse and Roubini (2009) show, it is the joint contribution of multiple factors that will help determine whether a country is likely to defaults. Analyzing any variable in isolation will yield inconclusive, at best, and misleading, at worst, results.

Therefore, after showing that collapses in public revenues and default are correlated, no matter what estimator is used, I introduce four sets of controls both independently and combined. These account for predictors of default found to be important in previous work (Eichengreen and Portes, 1986; Detragiache and Spilimbergo, 2001; Manasse and Roubini, 2009; Catão and Milesi-Ferretti, 2014), for which data is available. First, I test



(a) Tax revenues and public expenditure 1926-1934



(b) Primary balance over GDP, 1927-33

Figure 3.7: Fiscal policy and default

1929=1. Source: author's calculations. The primary balance is calculated as $(TaxRevenue - Expenditure + DebtService)/NGDP$. See Appendix 4.B for details on the sources.

whether the severity of the slump was related to the probability and size of default, and whether this explains away the effect of the contraction in public revenues. Second, I test whether default was correlated with potential reputational costs by controlling for countries' reliance on the external sector in terms of both trade and finance. Third, I study the predictive power of the size and composition of both public and private debts for default. Finally, I test whether the fiscal and monetary policies carried out by governments had any relation with the default outcome. Controls are generally introduced as natural

logarithms to make interpretation simpler. Those which can take zero values, however, are introduced as standard variables.

The basic model in my estimations is outlined in Equation 3.1.

$$DefaultSize_{i,t} = \alpha + \theta \frac{PublicRevenue_{i,t-1}}{PublicRevenue_{i,1929}} + \mathbf{x_{i,t-1}}\beta + \epsilon_{i,t} \tag{3.1}$$

where $\frac{PublicRevenue_{i,t-1}}{PublicRevenue_{i,1929}}$ is my public revenue loss indicator, $\mathbf{x}$ is a vector of controls and ϵ is the idiosyncratic error term. To reduce the risk of reverse causality, all regressors are entered with a lag, as is commonly done in the literature.

Assuming all the usual Gauss-Markov conditions are met, OLS yields consistent estimates of the marginal effects of the explanatory variables on default size, even if the dependent variable is constrained in the 0-1 interval. However, the linear model suffers from well known problems deriving from the fact that the conditional mean of the dependent variable is assumed to be linear in the regressors. This means that the predicted default size can lie outside the 0-1 interval. Nonetheless, a linear model represents a useful approximation for two reasons: 1) straightforward interpretation of the coefficients 2) the possibility of including fixed effects in a simple way.

To overcome the issues associated with linearity, I also run this basic model using the Probit and Tobit estimators. Probit is often the model of choice in the literature, however it is not fully adequate in this context since my dependent variable is not a binary crisis/no crisis indicator, but a continuous variable. Tobit, instead, fits this application well since it is a corner solution response model where the dependent variable is bounded by one or two corner values and can have positive probability mass at these (Wooldridge, 2010). In my case the corners are 0 (no default) and 1 (complete default).[26] I also employ the Pseudo Poisson Maximum Likelihood (PPML) estimator designed by Santos Silva and Tenreyro (2006) to deal with the nonlinearity introduced by having logarithms in the estimation.

The results of all the these estimations are vulnerable to omitted variable bias, since I do not control for potentially crucial unobserved country characteristics through fixed effects. Unfortunately, consistent estimators are available only for random effects effects Tobit, while they do not exist for fixed effects Tobit. The former imply the implausible assumption that the time-invariant country characteristics are uncorrelated with the other regressors. However, a procedure to run a Tobit model with correlated random effects

[26] Like all non linear models, the estimated Probit and Tobit coefficients cannot be interpreted as marginal effects as one would do with OLS or other linear models. The marginal effects need to be computed for each level of the explanatory variables, but their sign and significance can be interpreted just as in the linear case.

(CRE), which imply only somewhat stricter assumptions than a fixed effects model, is available (Wooldridge, 2005, 2010). I present its results in Appendix 3.A. The estimates are very similar qualitatively and quantitatively to those of the linear estimations.[27]

As a baseline methods, however, I prefer employing both standard and dynamic linear panel data estimators to account for country fixed-effects in a robust and straightforward fashion. In particular, I employ the within (FE) estimator as the principal method[28] and the the dynamic system Generalized Method of Moments (GMM) estimator (Blundell and Bond, 1995) for robustness. The difference GMM (Arellano and Bond, 1991) yields similar results.[29]

The model takes the following form with fixed effects:

$$DefaultSize_{i,t} = \alpha + \theta \frac{PublicRevenue_{i,t-1}}{PublicRevenue_{i,1929}} + \mathbf{x_{i,t-1}}\beta + c_i + l_t + \epsilon_{i,t} \tag{3.2}$$

and, the following general form when dynamics are included:

$$DefaultSize_{i,t} = \alpha + \mathbf{A}(\ell)\{DefaultSize_i + \frac{PublicRevenue_i}{PublicRevenue_{i,1929}} + \mathbf{x_i}\} + c_i + l_t + \epsilon_{i,t} \tag{3.3}$$

where $\mathbf{A}$ is a matrix of polynomials in the lag operator, ℓ is an aribitrary number of lags, and c and l are country and time fixed effects respectively.

Apart from accounting for fixed effects, an important feature of panel data methods is that they exploit the time-series rather than cross-sectional variation. As mentioned in Section 3.3, this is an attractive feature in the context of this paper. Different accounting and reporting standards make the data imperfectly comparable across countries. By exploiting the time series variation, all one needs for consistent estimation is that accounting

[27] Greene (2004) has shown that the incidental variables problem that afflicts non-linear estimators when fixed-effects are introduced as simple dummies does not affect the slope of Tobit MLE estimates. However it shows up in the estimate of the variance. The bias appears to diminish quickly as T increases, but any inference based on this estimator remains doubtful, at best.

[28] This accounts for fixed effects by subtracting the average value from the variables.

[29] With a dynamic structure, standard fixed and random effects estimators are biased since the lagged dependent variable is correlated with the differenced error term. For this reason, a GMM estimator is necessary. This type of estimator uses longer lags of the variables to instrument the lagged variables. For this strategy, it is essential that the error be serially uncorrelated. Standard tests exist to verify whether this condition is met. The Arellano and Bond (1991) exploits the moments conditions generated by instrumenting differenced variables with longer lags of their levels without losing any observations (apart from the first) in the process. This is achieved by changing the number of instruments with the lags available. The Blundell-Bond (a.k.a system) GMM (Blundell and Bond, 1995) performs the estimation in levels with the lagged differenced variables used as instruments for the regressors. Compared to the difference estimator, this model uses some additional orthogonality conditions which improve the precision of the estimates when the autoregressive parameter (i.e. the coefficient of the lagged dependent variable) is close to one. However, this comes at the cost of additional assumptions. In particular, one has to assume that the dependent variable is mean stationary.

standards do not change over time for the same country.

A drawback of panel data methods is that time-invarying explanatory variables are lost and slow-moving ones are subject to a drastic reduction in their variability, leading to less precise estimates and, possibly, to coefficients appearing to be statistically insignificant when in fact they are not. Moreover, these models – like OLS – are linear and suffer from the shortcomings outlined above. As Wooldridge (2010) argues, both the linear and nonlinear approaches have advantages and drawbacks, which, at the the current state of statistical knowledge, cannot be overcome. He suggests reporting and drawing inference from both, as I do here.

The dynamic element of the GMM estimators is important because persistence in the case of defaults is a natural assumption. A country could be in default in a certain period simply because it was in default during the last period. The debt renegotiations that follow sovereign defaults are notoriously lengthy: even in the face of improving economic conditions a country might seek to restructure its obligations to obtain a reduction in the debt, while creditors might hold-up hoping for a better deal. The interwar period was no exception and some defaults that began in the 1930s were only fully resolved after the end of World War II. The German one, for example, was settled by the London Debt Agreement of 1953.

Including a lagged dependent variable in the estimation also drastically reduces the risk of feedback loops between default and the regressors. This is because defaults tend to have large macroeconomic repercussions, which would impact the explanatory variables, leading to reverse causality. However, even including a lagged default indicator does not solve the issue that the other regressors might be affected by the prospect of default during the run-up to a debt crisis. This problem is a fundamental limitation of most empirical studies of defaults and is one of the reasons why these tend to focus on prediction rather than causal relationships. To account for it as best as I can, I show that my results are robust to excluding the year a country enters default (see Appendix 3.A). In this way, the feedback loop between default and the other regressors is accounted for by the lagged default size indicator, while the immediate run-up to default is excluded, eliminating observations which could be affected by default-expectation-induced changes in the regressors. The possibility that default expectations affect the regressors more than two years in advance of default might still mean that the macroeconomic repercussions of default expectations are not accounted for. In this instance, the results can still be interpreted as insightful and robust conditional correlations, as in the existing literature.

As discussed above, the objective and approach of this paper is different from both previous studies on sovereign debt crisis in general and those focusing on the Great Depression. Compared to Eichengreen and Portes (1986), I have a wider set of controls, more sophisticated estimates of public debts, a sample that includes all key players and a panel data approach. Compared to studies on more recent crises, such as Detragiache and Spilimbergo (2001), Manasse and Roubini (2009) and Catão and Milesi-Ferretti (2014), I have a continuous rather than a binary outcome variable, which accounts for the dynamics of default over time and the existence of partial default. Additionally, the use of a dynamic methods represents a step forward in the attempt to minimize the issue of reverse causality. Furthermore, my objective is not maximizing the predictive ability of the model, as in these studies. The main objective of this work is to identify the impact of different macroeconomic and political variables on the probability and size of defaults, combining them as little as possible in composite indicators for which the contribution of each element cannot be disentangled. The Great Depression setting allows me to do this because this event represents a single and enormous common shock, which permits me to trace the evolution of variables over the course of the episode. Studies that aggregate default episodes over long stretches of history, subject to multiple and idiosyncratic shocks, cannot construct variables in such a way. Remarkably, however, the portion of the variance explained by my model is comparable to that of papers focusing on maximizing predictive power. Naturally, looking at a single default episode has the drawback of a small sample size and of more difficult generalization of the results to other default episodes.

3.4.2 Baseline Results

National-Provincial Defaults

Table 3.6, shows that, no matter what estimator is used, the size of national-provincial defaults and the public revenue loss – measured as the natural logarithm of the lagged level of tax revenues compared to 1929 – are strongly correlated. The estimates demonstrate that countries that saw their tax revenue deteriorate the least compared to the pre-crisis year of 1929 were less likely to default and to undergo larger defaults.

In Table 3.7 and 3.8, I retain the FE estimator, which balances efficiency and robustness, and insert the lagged controls one at a time. These follow the four categories outlined above.

I measure the severity of the slump (columns 1-3) in three different ways: the loss of nominal GDP, the loss in real GDP per capita and the loss of trade, all relative to 1929.

Table 3.6: National-provincial defaults & tax revenues

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	Probit	Tobit	PPML	FE	FD	DiffGMM	SysGMM
NatProvDefaultSize								
L.NatProvDefaultSize							0.881***	0.771***
							(0.0857)	(0.0502)
L.LnCentTax/CentTax29	-0.595***	-2.283***	-8.514*	-1.321***	-0.438***	-0.172*	-0.330***	-0.226***
	(0.133)	(0.742)	(4.782)	(0.308)	(0.129)	(0.0882)	(0.0826)	(0.0537)
Constant	0.109***	-1.067***	-3.753*	-1.975***	0.128***	0.0406***		0.0121
	(0.0391)	(0.243)	(1.963)	(0.268)	(0.0155)	(0.0119)		(0.0126)
Country fixed-effects					✓	✓	✓	✓
Observations	249	249	249	249	249	221	244	249
Number of countries	29	29	29	29	29	29	29	29
R-squared	0.171			0.095	0.107	0.030		
Robust standard errors in parentheses								
*** p<0.01, ** p<0.05, * p<0.1								

Time frame: 1927-36. L. stands for lagged. Countries included are: Argentina, Australia, Austria, Belgium, Bolivia, Brazil, Bulgaria, Canada, Chile, Colombia, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Netherlands, Norway, Peru, Poland, Romania, Sweden, Switzerland, United Kingdom, Yugoslavia. For the GMM estimations I use the 3rd, 4th and 5th lag of the explanatory variables as instruments. I also use the small sample correction, the twostep estimator and the orthogonal option to minimise the loss o observations in the presence of gaps in the data. Errors are clustered at country level

As expected all three indicators are negatively correlated with default size: countries whose economic outlook deteriorated the least, were less likely to default. However the nominal GDP and trade coefficients are more precisely estimated than the real GDP one, as evidenced by their strong statistical significance. This is presumably due to the fact that the nominal GDP and trade figures offer a more holistic picture of the slump than the real GDP figure. This is because the Great Depression was not a simple contraction in output, but was accompanied by a large deflation and a collapse in international trade, which the real GDP figures capture only partially.

I assess the predictive power of countries' reliance on the external sector in columns 4-7. In column 4, I control for the foreign share of the public debt, while in column 5 I proxy countries' external financing need using the trade balance over GDP. In column 6, instead, I control for the total borrowing of the economy in dollars – this includes both the public and the private sectors – normalized by GDP. Finally, in column 7, I control for the country's openness to trade, measured as the share of trade in GDP. The prior regarding the sign of the coefficients is unclear. On one hand, a higher reliance on the foreign sector should dissuade countries from defaulting due to the higher cost associated with being cut off from foreign markets for goods, services and capital. Moreover, a larger trade deficit would indicate that a country is able to attract capital from abroad, and is thus potentially more creditworthy in the eyes of investors. On the other hand, a higher

Table 3.7: National-provincial defaults, tax revenues & controls – FE, Part 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
NatProvDefaultSize									
L.LnCentTax/CentTax29	-0.219**	-0.332***	-0.176*	-0.380***	-0.342**	-0.326***	-0.220**	-0.378***	-0.481***
	(0.0933)	(0.108)	(0.0935)	(0.107)	(0.139)	(0.0932)	(0.100)	(0.117)	(0.128)
L.LnNGDP/NGDP29	-0.626***								
	(0.223)								
L.LnGDPPC/GDPPC29		-0.469							
		(0.309)							
L.LnTrade/Trade29			-0.339***						
			(0.0976)						
L.ForDebtShare				-1.617***					
				(0.361)					
L.TradeBal/GDP					3.651***				
					(0.954)				
L.lnDollarDebt/GDP						0.111*			
						(0.0552)			
L.Openness							-0.441***		
							(0.129)		
L.ShortDebtShare								0.580	
								(0.510)	
L.ShortDollarDebtShare									-1.247*
									(0.662)
Constant	0.0488*	0.101***	0.00608	0.880***	0.230***	0.467**	-0.496***	0.0367	0.147***
	(0.0242)	(0.0172)	(0.0319)	(0.170)	(0.0325)	(0.179)	(0.178)	(0.0853)	(0.0137)
Country fixed-effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	225	233	225	227	233	223	225	217	247
Number of countries	26	27	26	27	27	26	26	27	29
R-squared	0.157	0.100	0.257	0.247	0.181	0.112	0.258	0.130	0.124

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Time frame: 1927-36. L. stands for lagged. Countries included in columns 1,3, 6,7 are Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Netherlands, Norway, Poland, Romania, Sweden, Switzerland, UK and Yugoslavia; column 2 and 5 additionally include Peru; compared to column 1, columns 4 and 8 include Bolivia and Peru and, but exclude Yugoslavia; compared to column 1, column 9 includes Bolivia, Estonia, Peru.

share of foreign debt would translate into a larger welfare gain due to more foregone payments abroad in case of default and a higher external financing need could be a sign of fragility, which would make default more likely. The evidence lends some support to both channels, but is far stronger for the former: openness, the foreign debt share and trade deficits are strongly negatively associated with default size. However the dollar debt-to-GDP ratio is positively associated with default, potentially pointing to excessive borrowing from abroad. The latter result, however, is only weakly significant.

I study the predictive power of the size and composition of the public (and private) debt burden on default using six different indicators starting from column 8 in Table 3.7 and ending with column 4 in Table 3.8. In order to investigate whether difficulties in

Table 3.8: National-provincial defaults, tax revenues & controls – FE, Part 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NatProvDefaultSize								
L.LnCentTax/CentTax29	-0.279***	-0.277*	-0.301***	-0.313***	-0.434***	-0.448***	-0.336***	-0.496**
	(0.0506)	(0.151)	(0.0772)	(0.0981)	(0.129)	(0.110)	(0.115)	(0.196)
L.LnCentralDebt/GDP	0.271***							
	(0.0942)							
L.LnTotalDebt/GDP		0.217**						
		(0.0902)						
L.LnDebtService/GDP			-0.0503					
			(0.0808)					
L.LnDomYieldSpread				0.0229				
				(0.0330)				
L.Polity2					-0.0101			
					(0.0223)			
L.FiscBalance/GDP						-0.741		
						(0.788)		
L.OnGold							-0.267***	
							(0.0710)	
L.LnGoldReserves/GDP								-0.0450
								(0.0383)
Constant	0.411***	0.211***	-0.0940	0.111***	0.165*	0.0420	0.282***	-0.114
	(0.105)	(0.0554)	(0.311)	(0.0227)	(0.0838)	(0.0307)	(0.0443)	(0.136)
Country fixed-effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	209	172	207	187	249	205	249	192
Number of countries	25	22	24	24	29	26	29	22
R-squared	0.183	0.136	0.085	0.093	0.115	0.169	0.254	0.119

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Time frame: 1927-36. L. stands for lagged. Countries included in column 1 are Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Netherlands, Norway, Poland, Romania, Sweden, Switzerland and UK; column 2 excludes Austria, Chile and Greece and Romania; compared to column 1, column 3 excludes Romania while column 4 excludes Bulgaria and Ireland but includes Yugoslavia; compared to column 1, column 5,7 includes Bolivia, Estonia, Peru and Yugoslavia while column 6 includes just Yugoslavia; column 8 compared to column 1 excludes Chile, Greece and Romania.

rolling over the debt – or liquidity pressures – might help predict defaults, I employ the short-term public debt and the short-term public and private dollar-denominated debt as shares of their respective totals (columns 8 and 9 in Table 3.7). The debt service variable in column 3 of Table 3.8 is also a measure of liquidity pressure as it represents funds a country needs to raise within a short-time horizon, but can also be interpreted as a more general measure of the debt burden. I find no systematic association between short-term debts and default. On the contrary, they appear to be weakly negatively associated in the case of short-term dollar liabilities.

The central and total (central plus local) public debt indicators, instead, are positively associated with default (columns 1 and 2 in Table 3.8). This finding is in line with theory and the conventional wisdom regarding default. However, it disappears in the dynamic

analysis below casting doubts on its robustness. Neither the public debt service (column 3) as a share of GDP nor the (domestic) yield spread vis-a-vis the USA (column 4) – which can also help capture expectations – are robustly associated with default.[30]

I control for differences in fiscal policy in column 6 using the fiscal balance as a share of GDP and proxy for monetary policy by introducing a gold standard membership dummy (column 7), and the size of gold reserves as a share of GDP (column 8).[31] The direction of fiscal policy does not appear to be robustly associated with default. Gold standard membership instead helps to reliably predict default. This is unsurprising: a default while still on gold would almost certainly have led to capital flight. Countries, instead, tended to go off gold, particularly by introducing exchange controls, before defaulting. Gold reserves, often cited as a leading indicator of the ability to service foreign debts, possess the right sign, but are not strongly enough associated with national-provincial defaults to appear as significant. Finally, column 5 of Table 3.8 shows that no systematic differences in defaults existed between more or less democratic countries, as determined by the Polity2 score from the Polity IV database (Marshall and Jaggers, 2005).

The tax revenue loss indicator is strongly negatively correlated with default non matter what control is added. Moreover, its coefficient is quantitatively similar across all specifications: a 10% larger ratio of current tax revenues compared to 1929, is associated with 2-5 percentage points smaller default.[32]

In Table 3.9, I combine the variables that emerged as statistically related to default in the previous analysis, while also adding time-fixed effects to account for common shocks and enhance the robustness of the results. The various specifications are as follows. In column 1, I control for the deterioration of trade and nominal GDP simultaneously. In column 2, I also control for gold standard membership. In column 3, instead, I combine information about the reliance of the economy on the external sector by controlling simultaneously for the foreign share of the public debt, the trade balance, the size of the dollar

[30] These results may appear surprising at first: one would expect the debt service and one of its main components – the yield – to be positively associated with default. However, the debt service naturally decreases with default given that the latter implies, by its very definition, a reduction in the former. Inserting the variable with a lag is not enough to account for this source of reverse causality. However, when I account for the feedback loop between default and debt service, using a dynamic estimation, the coefficient has the expected sign. In the case of the spread, the sign is correct but the coefficient is insignificant.

[31] Data on foreign exchange reserves is very fragmentary so I rely on gold reserves only. However, Lindert (1969) shows that in 1928, at their probable peak, foreign exchange reserves made up only around 24.5% of total reserves.

[32] Given that for defaulters tax revenues in 1933 were on average 0.7 of their 1929 level, a 10% smaller decrease in revenues translates into a 0.07 larger ratio of current tax revenues compared to 1929, well within the observed variation in the data, which is between 0.13 and 1.2.

Table 3.9: National-provincial defaults, tax revenues & controls combined – FE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NatProvDefaultSize							
L.LnCentTax/CentTax29	-0.228**	-0.230**	-0.200**	-0.197**	-0.264***	-0.259***	-0.235***
	(0.0963)	(0.100)	(0.0791)	(0.0821)	(0.0535)	(0.0594)	(0.0788)
L.LnNGDP/NGDP29	0.262	0.205					
	(0.303)	(0.306)					
L.LnTrade/Trade29	-0.303	-0.244					0.490
	(0.221)	(0.217)					(0.340)
L.ForDebtShare			-0.802	-0.791			-0.841
			(0.531)	(0.519)			(0.523)
L.TradeBal/GDP			0.932	0.743			0.726
			(0.788)	(0.835)			(0.740)
L.lnDollarDebt/GDP			0.105**	0.0997*			0.0690
			(0.0468)	(0.0498)			(0.0438)
L.Openness			-0.327	-0.286			-0.939**
			(0.204)	(0.206)			(0.371)
L.OnGold		-0.110		-0.117		-0.179**	-0.118*
		(0.0726)		(0.0689)		(0.0695)	(0.0659)
L.ShortDollarDebtShare					-0.311	-0.449	-0.110
					(0.463)	(0.412)	(0.459)
L.LnCentralDebt/GDP					0.113	0.124	0.284*
					(0.102)	(0.0965)	(0.141)
Constant	-0.00341	0.0714	0.369	0.474	0.170	0.320**	0.0669
	(0.0587)	(0.0658)	(0.402)	(0.409)	(0.119)	(0.134)	(0.509)
Country fixed-effects	✓	✓	✓	✓	✓	✓	✓
Time fixed-effects	✓	✓	✓	✓	✓	✓	✓
Observations	225	225	203	203	207	207	200
Number of countries	26	26	25	25	25	25	25
R-squared	0.330	0.344	0.396	0.411	0.322	0.361	0.455

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Time frame: 1927-36. L. stands for lagged. Countries included in columns 1,2 are Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Netherlands, Norway, Poland, Romania, Sweden, Switzerland, UK and Yugoslavia; columns 3-7 exclude Yugoslavia.

debt relative to GDP and the trade openness. In column 4, I add the on gold indicator. In columns 5 and 6, I control simultaneously for the short term-share of dollar denominated debts and the size of central government debts relative to GDP; in column 6, I also control for gold standard adherence. In column 7, I combine all controls, except the nominal GDP indicator (including it does not change the results). Gold standard membership, the debt burden and openness emerge as the key factors in predicting the incidence and size of national-provincial defaults in this comprehensive specification.

In all specifications, my main variable of interest – public revenue loss – emerges as strongly negatively associated with default. The size of the coefficient is also similar to the estimation above: a 10% smaller deterioration in revenues led to an approximate 2-

3 percentage points smaller default. In a counterfactual world where revenues in 1933 were at their 1929 level, the average national-provincial default would have been up to 11 percentage points smaller, everything else equal.

Municipal Defaults

The structure of the analysis for municipal defaults mirrors that of national-provincial defaults. I first test the correlation between revenue loss and default. I then introduce the controls one at a time and finally combine them. In this section, the revenue loss is measured as the natural logarithm of the level of local government financing compared to 1929. I use government financing – which includes non tax revenue and long term (over 1 year) borrowing – rather than just tax revenues due to a much wider availability of the former compared to the latter. In principle, the two should be very closely related, particularly after the widespread collapse of financial markets after 1929, which curtailed the availability of borrowing.

The municipal data is still less complete than the national-provincial data. This is partly due to data availability, and partly due to the fact that in some countries, such as the United Kingdom and Italy, local governments did not borrow in dollars in this period. This reduces the number of countries in the analysis to 16-19 compared to the 22-29 above.

Table 3.10: Municipal defaults & local government financing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	Probit	Tobit	PPML	FE	FD	DiffGMM	SysGMM
MunDefaultSize								
L.MunDefaultSize							0.605***	0.752***
							(0.123)	(0.0887)
L.LnLocGovFin/LocGovFin29	-0.370*	-2.239***	-3.769***	-3.483***	-0.302	-0.0984	-0.324*	-0.235
	(0.198)	(0.859)	(0.988)	(0.929)	(0.175)	(0.114)	(0.172)	(0.157)
Constant	0.0703**	-1.524***	-2.532***	-2.969***	0.0749***	0.0454**		0.0329*
	(0.0282)	(0.234)	(0.960)	(0.408)	(0.0118)	(0.0195)		(0.0179)
Country fixed-effects					✓	✓	✓	✓
Observations	140	140	140	140	140	128	126	140
Number of countries	19	19	19	19	19	19	19	19
R-squared	0.105			0.059	0.083	0.008		
Robust standard errors in parentheses								
*** p<0.01, ** p<0.05, * p<0.1								

Time frame: 1927-36. L. stands for lagged. Countries included are: Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Estonia, Finland, France, Germany, Japan, Netherlands, Norway, Poland, Switzerland. For the GMM estimations I use all lags after the 2nd of the explanatory variables as instruments. I use the twostep estimator and the collapse orthogonal option to minimise the loss o observations in the presence of gaps in the data and reduce the number of instruments, see Roodman (2009).

Table 3.10 demonstrates that revenue loss and default were related also at the munic-

ipal level. This result is obtained using both linear – OLS – and non-linear methods – Probit, Tobit, PPML – as well as standard and dynamic panel data methods – fixed effects, first differences, difference and system GMM. Although the coefficients are not always statistically significant at conventional levels, their magnitudes are remarkably similar across specifications and also relative to the national-provincial estimates above. This suggests that the smaller number of observations might be the cause of the lack of significance, while the true relationship between the variables is qualitatively and quantitatively similar to the one identified above.

The same observation holds for Tables 3.11 and 3.12, in which I introduce the controls one at a time. The local finance coefficient is negative in all specifications and remarkably stable in magnitude.

Table 3.11: Municipal defaults, government financing & controls – FE, Part 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
MunDefaultSize									
L.LnLocGovFin/LocGovFin29	-0.157	-0.173	-0.163	-0.247*	-0.296*	-0.234	-0.200*	-0.195	-0.264
	(0.142)	(0.163)	(0.114)	(0.136)	(0.149)	(0.152)	(0.110)	(0.156)	(0.156)
L.LnNGDP/NGDP29	-0.566**								
	(0.259)								
L.LnGDPPC/GDPPC29		-0.686							
		(0.413)							
L.LnTrade/Trade29			-0.279**						
			(0.0976)						
L.TradeBalance/GDP				2.573*					
				(1.438)					
L.ForDebtShare					-1.043				
					(0.836)				
L.lnDollardebt/GDP						0.0911			
						(0.0755)			
L.Openness							-0.367**		
							(0.131)		
L..ShortDebtShare								1.629***	
								(0.486)	
L.ShortDollarDebtShare									-1.424
									(1.353)
Constant	0.0238	0.0463*	-0.00813	0.143***	0.577	0.346	-0.409**	-0.153*	0.0899***
	(0.0322)	(0.0251)	(0.0373)	(0.0315)	(0.396)	(0.216)	(0.180)	(0.0782)	(0.0156)
Country fixed-effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	135	134	135	135	131	135	135	126	141
Number of countries	18	18	18	18	18	18	18	18	19
R-squared	0.190	0.138	0.284	0.160	0.142	0.089	0.275	0.369	0.089
Robust standard errors in parentheses									
*** p<0.01, ** p<0.05, * p<0.1									

Time frame: 1927-36. L. stands for lagged. Countries included in column 1-7 are Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Japan, Netherlands, Norway, Poland, Switzerland; column 8 addtionally includes Estonia.

Table 3.12: Municipal defaults, government financing & controls – FE, Part 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MunDefaultSize								
L.LnLocGovFin/LocGovFin29	-0.298	-0.235	-0.313	-0.286	-0.329*	-0.225*	-0.306**	-0.189
	(0.202)	(0.163)	(0.183)	(0.187)	(0.175)	(0.127)	(0.131)	(0.155)
L.LnLocalDebt/GDP	0.0988							
	(0.0782)							
L.lnTotalDebt/GDP		0.189						
		(0.136)						
L.LnDebtService/GDP			-0.00197					
			(0.0873)					
L.LnDomYieldSpread				-0.0114				
				(0.0289)				
L.Polity2					-0.0151			
					(0.0310)			
L.NatProvDeafultSize						0.508**		
						(0.204)		
L.OnGold							-0.244**	
							(0.0926)	
L.LnGoldReserves/GDP								-0.0810***
								(0.0271)
Constant	0.284	0.209**	0.0726	0.101***	0.157	0.0394**	0.217***	-0.230**
	(0.166)	(0.0957)	(0.339)	(0.0162)	(0.172)	(0.0171)	(0.0492)	(0.106)
Country fixed-effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	119	119	133	110	140	140	140	128
Number of countries	16	16	18	17	19	19	19	17
R-squared	0.139	0.168	0.086	0.075	0.107	0.328	0.310	0.167
Robust standard errors in parentheses								
*** p<0.01, ** p<0.05, * p<0.1								

Time frame: 1927-36. L. stands for lagged. Countries included in columns 1,2 are Australia, Belgium, Brazil, Bulgaria, Canada, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Japan, Netherlands, Norway, Poland and Switzerland; columns 3,4 additionally include Austria and Chile, but column 4 excludes Bulgaria; compared to column 1, columns 5,6,7 include Austria, Chile and Estonia; compared to column 1, column 8 includes Austria.

Similarly to the national-provincial results above, columns 1-3 of Table 3.11 demonstrate that the loss of nominal GDP, real GDP per capita and trade are associated to default separately from the loss of local government finance. Although the signs of the coefficient is in line with those found above, I cannot identify a statistically significant relationship between the foreign share of the debt and default (column 4). The same goes for the dollar dominated debt as a share of GDP (column 5). However, trade openness (column 6), as in national-provincial case, emerges as strongly negatively related to default at the municipal level. Thus, reliance on the external sector might have been an important driver of the decision to renege on foreign obligations for municipal authorities, presumably due a reputational mechanisms that would imply higher costs of being excluded from foreign markets following default. Column 7, suggests that the maturity structure of debts also played a role in municipal defaults. Countries with a higher share of short term public

debt were more likely to undergo larger municipal defaults. No similar effect can be found by looking at the short term share of economy-wide dollar dominated debt (column 8).

In columns 1-4 of Table 3.12, I assess the predictive power of the debt burden on default, by controlling for for the size of local public debts as a share of GDP, the size of central plus local public debts as a share of GDP, the incidence of the debt service in GDP and the domestic yields spread (which serves as a proxy for the foreign yield spread, for which significantly less data is available). None of these indicators provide statistically significant additional information on the incidence of default. The same goes for the level of democracy, as measured by the Polity 2 score in column 5.

When I control for the incidence of national-provincial defaults in column 6, instead, I find that these help predict municipal defaults. This finding suggests some degree of spillover from the former to the latter. A fully fledged explanation is beyond the scope for this paper, but it is likely that, following the default of a higher level political entity the reputational repercussions of default would be muted for municipalities in case they also defaulted. Another potential explanation is contagion in the form of higher yield spreads or capital flight.

As for national-provincial defaults, I find that countries tended to go abandon the gold standard before defaulting (column 7). Finally, in column 8 I find that the magnitude of gold reserves – normalized by GDP – helps to predict default. This result contrasts with the one for national-provincial defaults for which reserves had no impact. This discrepancy is not difficult to rationalize given the historical context. Gold reserves were predominantly at the disposal of central governments and only subordinately of local governments. The seniority of national-provincial debts with respect to municipal debts was certified by renegotiations following default. It was common practice to prioritize the repayment of central and – to a lesser extent – provincial government debt compared to municipal debt. A clear example of this is the renegotiation of Brazilian public debts in the 1930s. Public liabilities were divided in categories commanding different degrees of seniority, with national and important provinces' debts occupying the top positions. Senior bond issues received pre-determined amounts of foreign exchange, while less senior ones were assigned residual amounts. Thus, for local governments the constraint on the ability to repay due to low gold (or foreign exchange) reserves was much more relevant than for national governments.

In Table 3.13, I combine the controls. The size of the coefficient on local government financing deterioration is once again stable across specification and comparable to previous

Table 3.13: Municipal defaults, government financing & controls combined – FE

	(1)	(2)	(3)	(4)	(5)	(6)
MunDefaultSize						
L.LnLocGovFin/LocGovFin29	-0.212	-0.234*	-0.255*	-0.223*	-0.178	-0.191
	(0.143)	(0.134)	(0.130)	(0.119)	(0.128)	(0.129)
L.LnNGDP/NGDP29	0.183					
	(0.381)					
L.LnGDPPC/GDPPC29	-0.236					
	(0.594)					
L.LnTrade/Trade29	-0.309**	-0.261	-0.231	-0.195	-0.164	-0.148
	(0.115)	(0.212)	(0.221)	(0.175)	(0.187)	(0.186)
L.Openness		0.0456	0.0583	0.0867	0.0531	0.0707
		(0.256)	(0.257)	(0.206)	(0.222)	(0.229)
L.ShortDebtShare		1.338***	1.216***	1.182**	1.257**	1.172**
		(0.409)	(0.418)	(0.424)	(0.441)	(0.454)
L.OnGold			-0.0657	-0.0358		-0.0484
			(0.0647)	(0.0542)		(0.0584)
L.NatProvDeafultSize				0.269*	0.199	0.184
				(0.147)	(0.124)	(0.129)
L.LnGoldReserves/GDP					-0.0290	-0.0331
					(0.0206)	(0.0239)
Constant	-0.0106	-0.143	-0.0600	-0.0424	-0.223	-0.169
	(0.0363)	(0.293)	(0.285)	(0.230)	(0.235)	(0.239)
Country fixed-effects	✓	✓	✓	✓	✓	✓
Observations	132	125	125	125	119	119
Number of countries	18	18	18	18	17	17
R-squared	0.307	0.519	0.526	0.579	0.519	0.523
Robust standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Time frame: 1927-36. L. stands for lagged. Countries included in columns 1-4 are Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Japan, Netherlands, Norway, Poland and Switzerland. Columns 5 and 6 exclude Chile. I do not include time fixed-effects due to the already limited number of degrees of freedom available. Quantitatively he results are similar, but standard errors are mucb larger if these are included.

estimations. However, it is statistically significant at conventional levels only in three specifications out of six. It should be noted, however, that the coefficient does not lose its significance because of the inclusion of other statistically significant regressors. This suggests a simple issue of noise in the data combined with a relatively small number of observations and some multicollinearity. The only other variable that is consistently significant across all specifications is the share of short term debts.

3.4.3 Robustness: dynamic estimation

In this section, I rerun the analysis above using dynamic panel data methods. The advantages (and disadvantages) of this methodology are outlined in Section 3.4.1 above. Here, it

is sufficient to say that these estimations are very tough tests to determine the predictive power of public revenue loss for defaults in the interwar period.

National-Provincial Defaults

I begin the analysis by introducing one control at a time, as above; the results are reported in Appendix 3.A. From the coefficient estimates, is is immediately clear that defaults are, indeed, persistent. The autoregressive parameter is large – between 0.714 and 0.921 – and strongly statistically significant. However, it is statistically different from 1 indicating that the dynamic process of default requires additional information to be fully explained.

Table 3.14: National-provincial defaults, tax revenues & controls combined – sysGMM

	(1)	(2)	(3)	(4)	(5)	(6)
NatProvDefaultSize						ex. 1^{st} year default
L.NatProvDefaultSize	1.117***	0.317	0.640	1.343***	0.868***	0.855***
	(0.365)	(0.252)	(1.272)	(0.456)	(0.214)	(0.142)
L.LnCentTax/CentTax29	-0.794**	-0.444*	-0.585**	-0.656**	-0.436***	-0.518***
	(0.360)	(0.244)	(0.244)	(0.285)	(0.135)	(0.143)
L.LnTrade/Trade29	0.355	-0.377***	-1.107	0.363	-0.103	-0.0364
	(0.240)	(0.143)	(0.795)	(0.349)	(0.423)	(0.253)
L.LnDebtService/GDP	0.835	0.541	0.194	0.682	0.540**	0.0376
	(0.582)	(0.436)	(0.330)	(0.791)	(0.250)	(0.165)
L.ForDebtShare		0.774*			0.226	-0.153
		(0.423)			(0.271)	(0.339)
L.Openness			1.744		0.00242	0.135
			(1.928)		(0.604)	(0.424)
L.LnCentralDebt/GDP				0.285	-0.405	0.200
				(0.837)	(0.303)	(0.129)
Constant	3.482	1.647	2.907	3.128	1.473	0.574
	(2.574)	(1.309)	(3.726)	(2.770)	(1.367)	(0.450)
Country fixed-effects	✓	✓	✓	✓	✓	✓
Time fixed-effects	✓	✓	✓	✓	✓	✓
Observations	207	197	207	202	197	190
Number of countries	24	24	24	24	24	24
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Time frame: 1927-36. L. stands for lagged. Countries included in columns 1-3 and 6 Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Netherlands, Norway, Poland, Sweden, Switzerland, United Kingdom. I use the 3rd, 4th and 5th lag of the explanatory variables as instruments. I use the twostep estimator and the collapse and orthogonal options to minimise the loss o observations in the presence of gaps in the data and reduce the number of instruments, see Roodman (2009). Standard errors are clustered at the country level.

In Table 3.14, I combine the regressors that emerged as significant in this previous estimation and also include statistically significant regressors from the fixed effects estimation. As above, I include time fixed effects and cluster errors at country level. The results are also robust to two-way clustering at year and country level. In the last column,

I exclude observations for the first year a country enters default to minimize the possibility of a feedback loop between default (and default expectations) and the other regressors.

The public revenue loss indicator once again emerges as statistically negatively associated with default. It is also substantially larger than in the fixed effects estimation above. The impact of a 10% smaller contraction in tax revenues compared to 1929 is now estimated to a have led to a 4-8 percentage points smaller national-provincial default on average. These magnitudes are similar to those obtained with the CRE Tobit estimator presented in Appendix 3.A, which range between 4.7 and 6.5 percentage points for average partial effects. This indicates that approximating the coefficients linearly leads to fairly accurate results.

No other regressor consistently and reliably helps to predict default. Even the autoregressive coefficient is not statistically significant in all specifications. Thus, the results once again strongly support the idea that fiscal fragility, as identified by the loss of public revenue experienced by countries during the Depression, was a leading factor in the defaults. Notwithstanding different estimators, controls, specifications and the noisy raw data, this result emerges clearly throughout the whole analysis.

Municipal Defaults

In Table 3.15, I combine the control variables, which emerged as significant in previous municipal estimations. In column, 1 I control for the deterioration of economic conditions using the ratio of real GDP per capita relative to 1929, while also controlling for gold standard membership. In column 2, I add the short term share of public debts and the amount of gold reserves relative to GDP. In column 3, I retain the GDP and reserves controls and add the degree of trade openness, while in column 4 I retain the GDP variable while controlling for the incidence of national-provincial defaults. Finally, in column 5, I introduce all controls at once. None of these variables emerges as statistically associated with default in a dynamic framework.

As outlined above, the results for municipal defaults are less precisely measured due to the lower number of observations. However, once again the public revenue loss coefficient is consistent in magnitude across specifications and similar to the results for both municipal and national-provincial defaults presented above. Moreover, it is only insignificant when all controls are added at the same time, and none of these come out as significant either, pointing to collinearity and small sample issues. I also find (not shown here) that when the loss of local public revenues is proxied by the central government's tax revenue loss – for

Table 3.15: Municipal defaults public revenues & controls combined – sysGMM

	(1)	(2)	(3)	(4)	(5)
MunDefaultSize					
L.MunDefaultSize	0.595***	0.582*	0.860***	1.617***	0.552
	(0.106)	(0.348)	(0.173)	(0.469)	(0.768)
L.LnLocGovFin/LocGovFin29	-0.389***	-0.245**	-0.259*	-0.205**	-0.235
	(0.122)	(0.107)	(0.141)	(0.0932)	(0.194)
L.LnGDPPC/GDPPC29	0.0796	-0.238	-0.0619	-0.325	-0.0710
	(0.221)	(0.455)	(1.008)	(0.844)	(0.721)
L.OnGold	-0.145	-0.00734			0.0242
	(0.0999)	(0.0577)			(0.124)
L.ShortDebtShare		1.458			1.174
		(1.078)			(0.929)
L.LnGoldReserves/GDP		-0.0161	-0.00393		0.00534
		(0.0332)	(0.0233)		(0.0708)
L.Openess			-0.0904		-0.0716
			(0.118)		(0.186)
L.NatProvDefaultSize				-0.640*	-0.0415
				(0.383)	(0.479)
Constant	0.109	-0.249	-0.124	0.00350	-0.219
	(0.0763)	(0.253)	(0.151)	(0.0439)	(0.345)
Country fixed-effects	✓	✓	✓	✓	✓
Observations	132	117	126	132	117
Number of countries	18	17	17	18	17
Standard errors in parentheses					
*** p<0.01, ** p<0.05, * p<0.1					

Time frame: 1927-36. L. stands for lagged. Countries included in columns 1 are Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, japan, Netherlands, Norway, Poland and Switzerland; column 2,3 and 5 exclude Chile. I use all lags of the explanatory variables as instruments staring from the 2nd, in combination with the collapse and principal component analysis options to reduce the number of instruments. I use the twostep estimator and orthogonal option to minimise the loss o observations in the presence of gaps in the data, see Roodman (2009). Standard errors are clustered at the country level.

which data is more widely available – the coefficient is consistently statistically significant and negative across specifications and similar in magnitude to all other estimations.

3.5 Conclusion

In this paper, I have revised a crucial episode of the Great Depression and one of the most important economic events of the 20th century: the sovereign debt crisis of the early 1930s. The paper helps to fill an important gap in our understanding of the crisis by rigorously testing the most important hypotheses put forward in the historiography and the recent empirical and theoretical literature on defaults in general.

I present two fundamental innovations with respect to previous research. First, I construct a new data set on public debts. This features, for the first time, sub-national

bodies in a consistent manner for a substantial number of countries worldwide, and it allows me to study national-provincial and municipal defaults separately. Second, I employ a flexible and robust econometric strategy, which features both standard and dynamic panel estimations. The Great Depression context also allows me to assess the predictive power of various macroeconomics and political variables without combining them into composites, which would rule out the possibility of disentangling the predictive power of each indicator.

The main focus of the paper is the relationship between fiscal fragility – epitomized by the severe deterioration in public revenues experienced by a number of countries – and default. I find this to be a quantitatively important and robust factor in the interwar defaults. The fact that this result survives the inclusion of other indicators of macroeconomic health and country fixed effects suggests that this variable contains additional and useful information for predicting default, as also suggested by recent work (Tomz and Wright, 2007; Arellano and Bai, 2016). Supported by further evidence presented in my second paper (Chapter 4) I argue that this additional information is the inherent fragility of a country's fiscal system. In accordance with recent contributions highlighting the role of asymmetric information in sovereign debt crisis (Catão, Fostel, and Ranciére, 2011, Forthcoming), I it appears that this only became evident as the Great Depression unfolded, and is thus not captured by country fixed effects.

The introduction of this paper posed two questions. 1) Were the defaults the result of misjudgment on the part of creditors and opportunistic behaviour by borrowers or the inevitable result of the worldwide slump and other factors beyond the control of borrowing countries? 2) To what extent were the factors leading to default global in nature or specific to individual countries?

As regards the first question, I show that both global shocks and specific country circumstances mattered. The Great Depression was a global event, but the degree of its severity and the prior economic and political health of countries were wildly at variance. The first question is more difficult to answer. While I find that all the variables which help predict the defaults are economic, some of them are the result of political processes and constraints. In particular, the lower default likelihood for countries with a strong dependence on foreign financing and trade might indicate a prioritization of the internationally oriented sectors of the economy compared to the internally oriented ones. Thus, external shocks certainly played an important role, but so did political choices and constraints.

Above all, however, the radically different responses of countries' fiscal systems to the slump determined the incidence and size of defaults at both the national-provincial and

municipal level. Countries which saw the largest collapses in their public revenues as a consequences of the Depression experienced the largest defaults. Their attempts to run primary fiscal surpluses and continue servicing the debts were unravelled by deep fiscal crises.

Even though fiscal fragility emerges as the leading predictor of default in the interwar period, the comprehensive picture of the Great Depression debt crisis I offer in this paper suggests that monocausal interpretations of this event – which assign the key role to either "bad luck" or the irresponsible behaviour of borrowers, lenders and middle-men – should be taken with caution. The interwar debt crisis was a key event of the Great Depression and it influenced policy-makers, academics and the public opinion for decades thereafter. As it could be expected, it was a highly complex affair in which exogenous shocks and discretionary choices all played a role.

Appendices

3.A Additional analysis and robustness checks

3.A.1 Replicating Eichengreen and Portes

In what is still the reference study of defaults during the Great Depression, Eichengreen and Portes (1986) perform a cross-sectional regression by pooling data from 1934 to 1938. Their main variable of interest is the combined default size of all public bodies of a country as a share of their total dollar and sterling denominated debts. My sample differs from theirs, in that it only reaches 1936 and excludes Czechoslovakia, Colombia, Bulgaria, Mexico, Spain, Costa Rica and El Salvador. On the other hand, it includes major countries – Austria, Belgium, Canada, Ireland, Switzerland and the United Kingdom – which are absent from Eichengreen and Portes' analysis. Argentina, Australia, Brazil, Chile, Denmark, Finland, Germany, Greece, Hungary, Italy, Japan, Netherlands, Norway and Sweden are common to both samples.

Another difference between my and Eichengreen and Portes' analysis is that I only look at defaults in Dollar bonds. It is sensible to treat Dollar and Sterling defaults separately given that Eichengreen and Portes' own work (Eichengreen and Portes, 1988) shows that Sterling and Dollar bondholders were treated quite differently. The final difference with is that, lacking a reliable measure of terms of trade deterioration, I prefer using lagged deterioration in total trade instead. I also lack measures of notes in circulation and/or gold reserves for a number of countries , so I run the regressions without the gold coverage variable, but the qualitative results do not change if this variable is included. In any case, unlike for Eichengreen and Portes, this variable has the expected negative sign.

Table 3.16 presents the results of the two specifications proposed by Eichengreen and Portes estimated by both OLS and Tobit. The two specifications differ in the definition of trade deterioration used. My results do not fully match those of Eichengreen and Portes. This is probably due to the differences in the sample outlined above, and could indicate that their results are not very robust. In any case, the estimates need to be taken with a grain of salt due to major potential issues of endogeneity.

Unlike Eichengreen and Portes, I do not find a robust positive relationship between a higher foreign debt burden and default size. However, in the estimations performed in the paper, this result is partially recovered. I do find that an adverse shock to trade has the right sign in the first specification (colums 1 and 3) – countries which experienced a

Table 3.16: Replicating Eichengreen and Portes (1988)

	(1)	(2)	(3)	(4)
	OLS	OLS	Tobit	Tobit
VARIABLES	DefaultSize	DefaultSize	DefaultSize	DefaultSize
ForDebt/GDP	0.631	0.927**	-0.221	6.631
	(0.481)	(0.447)	(5.047)	(4.865)
Trade31/Trade29	-1.641***	-8.343*		
	(0.339)	(4.743)		
Trade28/GDP	-0.602	-19.99**		
	(0.387)	(8.033)		
%ΔDeficit29-31	0.0309***	0.0168***	2.642**	2.289*
	(0.00439)	(0.00534)	(1.281)	(1.359)
SouthAmerica	0.265***	0.335**	2.748**	2.564*
	(0.0824)	(0.147)	(1.231)	(1.299)
Australia	-0.714***	-0.827***	-10.84	-14.29
	(0.228)	(0.231)	(0)	(0)
L.Trade/Trade29		0.0794		5.870
		(0.357)		(5.560)
L.Trade/GDP		-2.163***		-42.15**
		(0.618)		(17.00)
Constant	1.437***	0.592**	9.449**	2.234
	(0.296)	(0.272)	(4.541)	(2.967)
Observations	62	62	62	62
R-squared	0.530	0.509		
	Robust standard errors in parentheses			
	*** p<0.01, ** p<0.05, * p<0.1			

L. stands for lagged. Countries included are Argentina, Australia, Austria, Belgium, Brazil, Canada, Chile, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Netherlands, Norway, Poland, Sweden, Switzerland, United Kingdom

larger deterioration in their trade compared to 1929 were more likely to default – but in the second specification (columns 2 and 4), the coefficient has an unexpected positive sign, but is insignificant.

Eichengreen and Portes, find a positive and insignificant effect of openness on the probability of default. Consistently with economic intuition, I find, instead, that more open countries were less willing to default in three specification out of four. This result is very much in line with economic theory and intuition and its interpretation is straightforward: countries whose GDP consisted for a greater share of imports and exports had more to lose from the fall in trade that normally accompanies defaults due to their direct (e.g. sanctions, embargoes) or indirect (lack of trade finance) effects.

I also find that my South America dummy (Eichengreen and Portes use a Latin America one, which they find to be insignificant) has explanatory power for default. Given the prevalence of default among South American nations, this is not surprising. The Australian

dummy is also found to be significant, although with an opposite sign. Eichengreen and Portes, who also find this result, argue that this could be due to the political and economic ties between Australia and its main creditors – the UK and the USA – which, everything else equal, would decrase the incidence of default.

Finally, Eichengreen and Portes find an inverse relation between default size and the change in the deficit, which would seem to indicate that an increase in "austerity" would be helpful for avoiding default. In my estimation, I find the opposite result, and this emerges quite strongly. This finding is supported by the data presented in the body of the paper and by the findings of my second paper (Chapter 4), which show hat "austerity" was – in many cases – not a choice but the only option for fiscally weak countries who were unable to borrow. These same countries, were more likely to default as demonstrated by the association between tax revenue deterioration and default found throughout the paper.

Compared to Eichengreen and Portes's estimations, this paper offers substantial advances. First, the inclusion of major borrowers such as Canada, means that a much greater share of US lending abroad is included in the analysis. Second, the focus on dollar denominated bonds reduces noise associated with the different treatment different classes of creditors received, which cannot be captured except in a bilateral analysis. Similarly, the distinction between national-provincial and municipal defaults allows me to treat different classes of debtors and creditors separately. Third, my analysis captures the full dynamic process of default: the analysis starts in 1927 when all countries except Brazil had not yet defaulted and follows them all the way to 1936 when Poland – the last country to do so – defaults. Fourth, I bring to the table newly collected data on public debts, including their composition and sub-national magnitudes as well as tax and other public revenues at both the national and local level. I also have additional data on variables such as nominal GDP and Openness, coming from recent work by other researchers. Finally, my panel and dynamic analyses account for unobserved and unobservable country characteristics and feedback loops between default and the other controls, radically reduce the risk of endogeneity and, thus, biased estimates.

3.A.2 Further robustness checks

In this section, I perform two robustness check. The most important consists in showing that the linear models used above approximate well the parameters of interest. I do so by using the Tobit estimator, which fits very well the nature of my dependent variable.

In fact, this model is able to capture both the fact that the default size indicator has probability mass at two values – 0 (no default) and 1 (complete default) – and the fact that it can take any value in between (partial default). Moreover, although non-linear models suffer from the well known incidental parameters problem when fixed effects are included in the regressions, Wooldridge (2005, 2010) has proposed a procedure to deal with this, which allows the researcher to included correlated random effects (CRE) in the analysis. This is a substantial step forward compared to random effects models which assume that the regressors and the unobserved country heterogeneity are unrelated. The procedure consists in conditioning the estimates on the average value of the regressors. The second robustness check consists in showing how the tax revenue coefficient estimates are affected when I introduce my controls one at a time in a dynamic setting

National-Provincial Defaults

I present the results of the CRE Tobit estimation for national provincial defaults in Table 3.17. The contraction in tax revenues emerges once again as a key factor in predicting default. I compute average partial effects (APE) as suggested by Wooldridge to gauge the quantitative significance of the coefficients and find that these are larger than the fixed effects and similar to the GMM estimates: they indicate that a 10% smaller contraction in tax revenues led to 4.7 and 6.5 percentage points smaller default. This suggests that the linear models yield good approximations of the coefficients. Additional results found above are also confirmed by this estimation. Countries more dependent on the external sector, as evidenced by the share of foreign debts, the trade balance and trade openness, were less likely to default. More indebted countries, as proxied by the dollar debt to GDP ratio, instead, were also more likely to default.

Tables 3.18 and 3.19 illustrate the impact of introducing the controls one by one a dynamic setting for national-provincial defaults. Public revenues loss emerges once again as strongly significant in all specifications – except for two – and similar in magnitude to the standard panel estimation above. The only two instances where the coefficient is not significant is when the dollar debt as a share of GDP and the short term dollar debt are introduced in the regression. In these instances the coefficients of the revenue loss variable more than halve while the standard errors become larger. This is puzzling because the variables have no statistically significant impact on default and are not strongly correlated with the revenue loss variable. It is therefore unclear why they would render it insignificant. Presumably this is simply an issue of noisy data.

Table 3.17: National-provincial defaults, tax revenues and controls combined – CRE Tobit

	(1)	(2)	(3)	(4)	(5)	(6)
NatProvDefaultSize						
L.LnCentTax/CentTax29	-3.134***	-2.689**	-3.208***	-3.105***	-3.761***	-3.710***
	(1.004)	(1.046)	(1.211)	(1.175)	(1.283)	(1.261)
L.LnNGDP/NGDP29	3.358*					
	(1.919)					
L.LnTrade/Trade29	-1.669**					4.410*
	(0.710)					(2.354)
L.ForDebtShare		-7.026**	-6.444**			-6.342**
		(3.015)	(3.035)			(3.133)
L.TradeBal/GDP		9.903	13.15*			14.52**
		(7.112)	(7.950)			(7.396)
L.lnDollarDebt/GDP		1.622**	1.736**			2.332**
		(0.783)	(0.702)			(1.046)
L.Openness		-4.299***	-4.267***			-8.821***
		(1.240)	(1.285)			(3.060)
L.OnGold			-0.237		0.468	-0.0502
			(0.984)		(1.013)	(0.765)
L.ShortDollarDebtShare				-4.685	-6.626	2.071
				(4.979)	(4.808)	(5.982)
L.LnCentralDebt/GDP				-0.0893	-0.183	0.323
				(0.630)	(0.678)	(0.774)
Constant	-10.71	-21.05	-20.81	-11.43	-9.429	-18.40
	(1,098)	(115.6)	(294.6)	(2,391)	(176.2)	(244.1)
Country fixed-effects	✓	✓	✓	✓	✓	✓
Time fixed-effects	✓	✓	✓	✓	✓	✓
Observations	225	203	203	207	207	200
Number of countries	26	25	25	25	25	25
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Time frame: 1927-36. L. stands for lagged. Countries included in columns 1 are Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Netherlands, Norway, Poland, Romania, Sweden, Switzerland, UK and Yugoslavia; columns 2-6 exclude Yugoslavia.

Regarding the other regressors, the economic contraction indicators emerge once again as reliable predictors of default. Unlike above, the impact of the debt service emerges as positively and significantly correlated with default. No other control is strongly enough correlated with default to emerge in a dynamic specification. In particular, the dollar debt, the public debt burden, the foreign debt share and the degree of trade openness all lose their statistical significance compared to the fixed effects estimation above, demonstrating the very high bar for robust estimation set by the GMM estimator.

Municipal Defaults

I run the same robustness check as above for municipal defaults. As pointed out above, the results for municipal defaults are less precisely measured due to the lower number of

Table 3.18: National-provincial defaults, tax revenues and controls – sysGMM Part 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NatProvDefaultSize								
L.NatProvDefaultSize	0.775***	0.781***	0.736***	0.760***	0.906***	0.760***	0.725***	0.921***
	(0.0594)	(0.0507)	(0.0821)	(0.0523)	(0.0610)	(0.0650)	(0.0597)	(0.0385)
L.LnCentTax/CentTax29	-0.134***	-0.110**	-0.183***	-0.199***	-0.0870	-0.214***	-0.202***	-0.0526
	(0.0506)	(0.0469)	(0.0444)	(0.0532)	(0.0724)	(0.0482)	(0.0482)	(0.0745)
L.LnNGDP/NGDP29	-0.229**							
	(0.109)							
L.LnGDPPC/GDPPC29		-0.407**						
		(0.163)						
L.LnTrade/Trade29			-0.0915**					
			(0.0395)					
L.ForDebtShare				0.0406				
				(0.0650)				
L.lnDollardebt/GDP					0.0245			
					(0.0174)			
L.lnOpenness						-0.0398		
						(0.0271)		
L.ShortDebtShare							0.118	
							(0.0853)	
L.ShortDollarDebtShare								0.461
								(0.373)
Constant	-0.0172**	-0.00678	-0.0253***	-0.0112	0.100	-0.0518	-0.00981	0.0301*
	(0.00852)	(0.00921)	(0.00888)	(0.0296)	(0.0610)	(0.0339)	(0.0121)	(0.0160)
Country fixed-effects	✓	✓	✓	✓	✓	✓	✓	✓
Number of countries	26	27	26	27	26	26	27	29
Observations	225	233	225	227	223	225	217	247

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Time frame: 1927-36. L. stands for lagged. Countries included in columns 1, 3, 5, 6 are Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Netherlands, Norway, Poland, Romania, Sweden, Switzerland, United Kingdom, Yugoslavia; columns 2, 4, 7 additionally include Peru; column 8 additionally includes Bolivia and Estonia. For the GMM estimations I use the 3rd, 4th and 5th lag of the explanatory variables as instruments. I employ the twostep estimator and the orthogonal option to minimise the loss o observations in the presence of gaps in the data, see Roodman (2009).

observations. The dynamic estimation confirms this: the coefficient for the revenue loss is statistically significant only in half of the specifications in Tables 3.20 and 3.21. However, once again the coefficient is consistent in magnitude across specifications and similar to the results for both municipal and national-provincial defaults presented above. The only exceptions are when the local and total debt variables are introduced in the regression, although these are not statistically significant themselves. In these instances the coefficient become much smaller. Presumably, the explanation is in multicollinearity and noisy data, combined with a relatively small sample. The hypothesis that a small sample size is behind the lack of significance is strengthened by the finding (not shown here) that when the loss of local public revenues is proxied by the central government's tax revenue loss, for which

Table 3.19: National-provincial defaults, tax revenues and controls – sysGMM Part 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NatProvDefaultSize								
L.NatProvDefaultSize	0.714***	0.845***	0.762***	0.719***	0.768***	0.786***	0.805***	0.781***
	(0.0726)	(0.0963)	(0.0986)	(0.0869)	(0.0600)	(0.0980)	(0.0558)	(0.0658)
L.LnCentTax/CentTax29	-0.184***	-0.286**	-0.128**	-0.251***	-0.144***	-0.146**	-0.189***	-0.129**
	(0.0351)	(0.135)	(0.0569)	(0.0560)	(0.0530)	(0.0631)	(0.0530)	(0.0640)
L.LnCentralDebt/GDP	-0.0121							
	(0.0157)							
L.LnTotalDebt/GDP		0.0530						
		(0.0338)						
L.LnDebtService/GDP			0.0354**					
			(0.0141)					
L.LnDomYieldSpread				0.01076				
				(0.02468)				
L.Polity2					-0.000781			
					(0.00479)			
L.FiscBalance/GDP						-0.860		
						(0.927)		
L.OnGold							0.000664	
							(0.0170)	
L.LnGoldReserves/GDP								-0.0209
								(0.0141)
Constant	-0.00733	0.0314	0.147***	0.001	0.0267	-0.00451	0.00419	-0.0654
	(0.0194)	(0.0290)	(0.0550)	(0.0160)	(0.0263)	(0.0223)	(0.0101)	(0.0434)
Country fixed-effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	209	172	207	187	249	205	249	192
Number of countries	25	22	24	24	29	26	29	22

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Time frame: 1927-36. L. stands for lagged. Countries include in column 1 are Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Netherlands, Norway, Poland, Romania, Sweden, Switzerland, United Kingdom; compared to column 1, column 2 excludes Austria, Chile and Romania; column 3 only excludes Romania; compared to column 1, column 4 includes Yugoslavia and excludes Bulgaria and Ireland; compared to column 1, columns 5 and 7 additionally include Bolivia, Estonia, Peru and Yugoslavia; while column 6 only additionally includes Yugoslavia; compared to column 1, column 8 excludes Chile, Greece, Ireland and Romania. For the GMM estimations I use the 3rd, 4th and 5th lag of the explanatory variables as instruments. I also use the small sample correction, the twostep estimator and the orthogonal to minimise the loss o observations in the presence of gaps in the data, see Roodman (2009).

data is more widely available, the coefficient is consistently statistically significant across specifications and similar in magnitude to all other estimations.

I find that no other variable introduced in the regressions is statistically associated with municipal defaults. Once again, this is probably due to the relatively small size of the sample.

Table 3.20: Municipal defaults, local government financing and controls – sysGMM Part 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MunDefaultSize								
L.MunDefaultSize	0.691***	0.900***	0.722***	1.062*	0.577***	0.652**	0.766***	0.855***
	(0.160)	(0.127)	(0.116)	(0.619)	(0.193)	(0.270)	(0.199)	(0.130)
L.LnLocGovFin/LocGovFin29	-0.274***	-0.196**	-0.349***	-0.272	-0.237	-0.475***	-0.321**	-0.241
	(0.0921)	(0.0899)	(0.110)	(0.694)	(0.232)	(0.173)	(0.139)	(0.187)
L.LnNGDP/NGDP29	-0.0250							
	(0.118)							
L.LnGDPPC/GDPPC29		-0.0484						
		(0.433)						
L.LnTrade/Trade29			-0.0584					
			(0.0643)					
L.ForDebtShare				-0.873				
				(4.472)				
L.LnDollarDebt/GDP					0.131			
					(0.133)			
L.Openness						-0.0519		
						(0.138)		
L.ShortDebtShare							0.720	
							(0.671)	
L.ShortDollarDebtShare								1.067
								(0.983)
Constant	-0.0115	-0.00504	-0.0222	0.437	0.395	-0.0560	-0.0986	0.0123
	(0.00732)	(0.0109)	(0.0138)	(2.273)	(0.357)	(0.160)	(0.0739)	(0.0268)
Observations	134	132	134	130	134	134	125	140
Number of countries	18	18	18	18	18	18	18	19
Standard errors in parentheses								
*** p<0.01, ** p<0.05, * p<0.1								

Time frame: 1927-36. Countries included in columns 1-7 are Australia, Austria, Belgium Brazil, Bulgaria, Canada, Chile Colombia, Czechslovakia, Denmark, Finland, France, Germany, Japan, Netherlands, Norway, Poland and Switzerland; column 8 additionally includes Estonia. I use all lags of the explanatory variables as instruments staring from the 2nd, in combination with the collapse and principal component analysis options to reduce the number of instruments. I use the twostep estimator and orthogonal option to minimise the loss o observations in the presence of gaps in the data, see Roodman (2009). Standard errors are clustered at the country level.

3.B Data Sources

In this Appendix, I provide detailed information on all the data collected. Firstly, however, I evaluate the reliability of the sources. In particular, I discuss the publications of the German Imperial Statistical Office (Statistiches Reichsamt) from which I extract a large share of the local public debt data.

During the interwar period the collection of economic and social statistics underwent a significant development in many countries, and Germany was no exeption. As Adam Tooze argues:

In the aftermath of the First World War enthusiasm for statistics and eco-

Table 3.21: Municipal defaults, local government financing and controls – sysGMM Part 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MunDefaultSize								
L.MunDefaultSize	0.627***	0.789***	0.823***	0.826***	0.590***	1.037***	0.682***	1.013***
	(0.108)	(0.118)	(0.119)	(0.124)	(0.132)	(0.248)	(0.137)	(0.295)
L.LnLocGovFin/LocGovFin29	-0.0518	-0.0331	-0.256***	-0.217	-0.169	-0.199	-0.522***	-0.447**
	(0.0558)	(0.0697)	(0.0968)	(0.212)	(0.380)	(0.158)	(0.201)	(0.205)
L.LnLocalDebt/GDP	0.0388							
	(0.0413)							
L.LnTotalDebt/GDP		0.0840						
		(0.0822)						
L.LnDebtService/GDP			-0.0165					
			(0.0436)					
L.LnDomYieldSpread				0.0392				
				(0.0556)				
L.Polity2					0.00251			
					(0.0549)			
L.NatProvDefaultSize						-0.0653		
						(0.161)		
L.OnGold							-0.129	
							(0.0812)	
L.LnGoldReserves/GDP								-0.00323
								(0.0577)
Constant	0.103	0.0752	-0.0509	0.0114	-0.00107	0.00455	0.0870	-0.0262
	(0.0996)	(0.0709)	(0.179)	(0.0333)	(0.397)	(0.0164)	(0.0689)	(0.212)
Observations	119	119	133	110	140	140	140	128
Number of countries	16	16	18	17	19	19	19	17

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Time frame: 1927-36. Countries included in columns 1,2 are Australia, Belgium, Brazil, Bulgaria, Canada, Colombia, Czechslovakia, Denmark, Finland, France, Germany, Japan, Netherlands, Norway, Poland and Switzerland; columns 3,4 additionally includes Austria and Chile, but column 4 excludes Bulgaria; compared to column 1 columns 5-7 include Austria, Chile and Estonia while column 8 includes Austria only. I use all lags of the explanatory variables as instruments staring from the 2nd, in combination with the collapse and principal component analysis options to reduce the number of instruments. I use the twostep estimator and orthogonal option to minimise the loss o observations in the presence of gaps in the data, see Roodman (2009). Standard errors are clustered at the country level.

nomic research was not confined to Weimar Germany. Indeed, in its early years Wagemann's institute [The Institut für Konjunkturforschung, founded in 1925] *drew its inspiration mainly from abroad. The major influences were the Harvard Committee for Economic Research, the NBER in Washington, the statistical initiatives of Herbert Hoover's Department of Commerce, and the Soviet Conjuncture Institute. In France and Britain also, there were calls for the government to play a greater role in data gathering and economic research. In the early 1920s private institutes for business-cycle research had been established in London and Paris along similar lines. However, no other European service ever achieved the size or authority enjoyed by the Berlin in-*

> *stitute. The Weimar republic was unique among west European states in the scale of its support for economic research which was not academic in character but designed specifically to supply information to government.*
>
> Tooze (1999)

Ernst Wagemann, the founder of the Institut für Konjunkturforschung, was also the head of the Statistiches Reichsamt from 1924 to 1933. The work of the institute and of the Statistical Office were closely tied together. Even after Wagemann lost his position as the head of the Statistical Office, he retained his lead at the Institute, with Hitler's personal backing and support. Under the Nazis, both the Institut für Konjunkturforschung and the Statistiches Reichsamt enjoyed a sort of golden age in terms of manpower and resources (Tooze, 1999), which resulted in the collection of a huge array of domestic and international statistics, a small part of which are used in the current paper.

Some of the original data reconstructions produced by the Institute were highly regarded and found their way into international publications. For example, the Institute reconstructed the international foreign debt position of a number of countries in 1932, and this was published in Moody's Investment Manuals unfailingly for a number of years thereafter.

However, the work of Rainer Fremdling – see, for example, Fremdling (2005, 2007) – has revealed that both the Institute and the Statistical Office worked under enrmous pressure from the authorities. This was mostly connected to the strategic planning for the war effort and the desire not to reveal too many details about the state of the German economy. For this reason, information was at times hidden through aggregation, misreported or not reported altogether. The data I use in this paper, however, would not be considered as sensitive information with regard to the war effort and is thus likely to be free of manipulation. Cross referencing with alternative, non-German sources (Moody's manuals in particular) has revealed no inconsistencies.

The United Nations (1948) volume, which I also use extensively, drew on the best available evidence of the time. The League of Nations regularly collected a wide range of international data in its Statistical Yearbooks, which the compilers of the UN volume organised and presented in a unified and coherent fashion. The issues with these data are discussed in detail below. In any case, the volume represents a remarkable, and transparent, effort to reconstruct a precise picture of central government debt in the interwar years.

My last major source of data are private publications created for the benefit of clients

and members of the general public by Moody's rating agency, the Institute for International Finance and the Corporation of Foreign Bondholders. The data found in these sources is often in agreement with data reported elsewhere. Moreover, the provision of accurate quantitative and qualitative information represented part the core business of these institutions. Moody's, in particular, was the first company in the world to produce foreign government bonds ratings in 1918, and were based directly on the data collected, as the information provided by Gaillard (2012) shows.

The biggest data contribution of this paper is a new public debt dataset. For central government debt this relies on data collected by the United Nations (1948), which presents some serious issues in terms of cross country comparability. The debt figures normally include the debt of state-owned enterprises, but there are some exception as in the case of the railways of Canada and Switzerland. More generally, the budgetary methods and accounting practices varied significantly across countries. Debt is sometimes shown as gross, sometimes as net with no consistent definition of these two terms across countries. Generally, net debt is the gross debt minus whatever claims against creditors – often the Central Bank – are held by the Treasury.

No consistent definition of short term debt existed either. The compilers of the volume settled for classifying debts with maturity of two years or less as short term debts. I retain this definition in the paper. The distinction between domestic and foreign debt was also often not the same across countries. Some classified their debts based on the currency of issues, some on the place of issuance, while other based it on the domicile of the creditors, whenever this was known. Conversions from foreign currency into domestic currency were also carried out in different ways. In most cases, the parities at which the debt was issued were used. In a few cases, current exchange rates were used, while in others the parity was adjusted periodically. Finally, in some instances war debts are included in the figures, while in others they are excluded. The inclusion or exclusion of these particular debts depended on the recognition of these obligations by the debtor state. Below, I discuss these issues on a country by country basis.

3.B.1 Common sources

Default size: I compute default size as the share of the principal of dollar bonds in default with regard to interest and/or interest payments. I construct separate measures for national-provincial and municipal levels of governments. I collect information on the timing and magnitude of bonds in default from Moody's (1933) page a17 for defaults up

to 1932, Moody's (1934) page a44 for 1933, Moody's (1935) page a45 for 1934, Moody's (1936) page a47 for 1935 and Moody's (1937) page a41-a44 for 1936. Outstanding dollar debts, instead, are from Lewis (1938).

Tax revenues and government financing: Tax revenues include all tax categories. Public financing is a composite, which also includes non-tax revenues and long-term (over 1 year maturity) borrowing. This item represented, in essence, the budgeted portion of government revenue and expenditure, with short term sources of finance (sometimes these were large) making up the slack. Most of the data comes from the German Imperial Statistical Office yearbooks, for both central and local level data. Details are provided in Appendix 4.B.

Dollar denominated debts: Information on total – public and private – dollar-denominated debts and their maturity composition on a annual basis are from Lewis (1938), Statistical Table 1, page 619-629. Lewis classifies short term debts as those with a maturity below 5 years. Unfortunately, disaggregated data is not available to make the short-term definition correspond to the below 2 years maturity used in the UN volume.

Gold Standard membership: The dates in which countries return to and leave the Gold Standard – either by devaluing or by introducing exchange controls – are from Crafts and Fearon (2013).

Nominal GDP: I use the nominal, non-PPP-adjusted GDP figures estimated by Klasing and Milionis (2014) for the period 1870-1949. These are based on Maddison's GDP estimates and obtained using the so-called "short-cut method". This method has a long history; Prados de la Escosura (2000) offers a detailed description. In essence, it exploits the relationship between PPP adjusted and non-PPP-adjusted GDP determined by the relative prices of traded and non-traded goods and the relative income level of the country compared to the benchmark country. In doing this, it makes use of the Balassa-Samuelson theorem. These data offer clear advantages compared to standard GDP figures in constructing measures of the debt burden, given that the latter are also in unadjusted nominal terms. They also capture an important additional feature of the Great Depression besides the contraction of output: the huge deflation that accompanied it.

Real GDP per capita: GDP per capita is taken from the Maddison's Project's latest update (Bolt and van Zanden, 2013).

Trade and openness: Trade and openness figures are from Klasing and Milionis (2014). The authors obtain the openness figures by combining their estimates of nominal GDP with trade data from Barbieri, Keshk, and Pollins (2009). The trade balance as a share

of GDP data are from Catão and Mano (Forthcoming).

Polity score: This is the Polity2 score from the POLITY IV database (Marshall and Jaggers, 2005). It is a combined score of autocracy and democracy (both measure between 0 and 10) and is obtained by subtracting the autocracy score from the democracy one. The two scores are weighed indicators of the competitiveness of political participation, the openness and competitiveness of executive recruitment and constraints on the chief executive.

3.B.2 Country-specific sources

Argentina

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 11 in millions of Pesos on 31 December 1914-1938. Data includes treasury bills obligations. Foreign debt shown at legal parity except for some shown at contractual rates. In the original figures, bonds held by the treasury have been discounted since 1927, I have kept them in to improve cross-country comparability. Short term debts in 1927 do not include oustanding unpaid commitments. Original source: Memoria del Departemento de Hacienda; Memoria de la Contaduria de la Nacion; El Ajuste de Los Resultados Financieros de los Ejercisios de 1928 a 1936, Buenos Aires; Direccion general de Estadistica, Informe No. 6, Series F., No. 2, Buenos Aires, 1923, Revista de Economia Argentina.

Local Government debt: I gathered this data from a variety of sources. No figure is available for 1928 and is interpolated as a the average between 1927 and 1929. No evident signs of jumps are present between the different series and the substantial overlap between the main data sources insures that the figures are consistent over time. The figure for 1927 is from Institute of International Finance (1927) – Credit Position of Argentina. and the 1929 figure is from Corporation of Foreign Bondholders (1929). The 1930 and 1931 figures are derived as the difference between the total public debt reported in Moody's (1931) page 10 and Moody's (1934) page 1667 and the central government debt reported in Moody's (1935). The 1932-1934 figures are taken from Moody's (1935) page 1771. The figures from 1935 to 1937 are taken from Statistisches Reichsamt (1939/40). The sub-national bodies covered in all sources are essentially the same. Minor differences exist between Institute of International Finance (1927), which includes the following municipalities: Buenos Aires, Rosaio, Cordoba, Santa Fe, Bahia Blanca, San Juan; and provinces: Buenos Aires, Santa Fe, Cordoba, Mendoza, Tucuman, Entre Rios, Santiago del Estero, Corrientes, San Juan,

Jujuy, San Luis, and the rest of the sources which include a number of additional minor bodies which, however, have a very small overall impact. These are the municipalities of: Rio Cuarto, Mendoza, Tucuman, Parana, Monteros, Santiago de Estero, Catamarca, Salta, La Rioja; and the provinces of: Salta, Catamarca, La Rioja and Jujuy. All figures are in millions paper Pesos.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 415, Jahreszahlen; 1934-1938 Statistisches Reichsamt (1939/40)page 258*-259*.

Australia

Central and local Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 15 in millions of Pounds on 30 June 1914-1938. Australia is one of the few instances where the original data included both central government (Commonwealth) and State debts. These data is included throughout the period even though the Commonwealth took over the outstanding debts of the States only on July 1 1929 under the terms of the Financial Agreement between the Commonwealth and the States. In the analysis, I have separated the two types of debts in order to insure a better comparability across countries. The separate state debts data comes from Statistisches Reichsamt (1939/40) (30 June 1926-1938). Original sources: The Budget, Finance Bulletins – Summary of Australian Financial Statistics, the Treasure's Statements of Receipts and Expenditure, Official Year-Book of the Commonwealth of Australia.

Gold reserves and notes in circulation: 1920-1933 Statistisches Reichsamt (1936a) page 526, Jahreszahlen; 1930-33 including gold held abroad; 1934-1938 Statistisches Reichsamt (1939/40) page 258*-259*.

Austria

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 19 in millions of Schillings on 31 December 1914-1938. Increase from 1930 partially due to the inclusion of some pre-1914 debts previously not recognised by the Austrian government. Original figures include some foreign loans issued in Austria, which I exclude to improve comparability across countries. Foreign debt data in foreign currency, 1924-31: converted into Shillings at the current exchange rates; 1932: converted at the official rate; 1933-37: converted at the average rates of private clearings on the respective dates. Debt service charged against ordinary revenue (excluding proceeds from loans). Original source: Bundes-Rechnungsabschluss der Republik Oesterreich,

Statistiches Handbuch fuer die Republik Oesterreich.

Local Government debt: no continuous series is available, a single data point is available for 1932 from Statistisches Reichsamt (1935).

Gold reserves: 1924-1933, Statistisches Reichsamt (1936a) page 208, Jahreszahlen; 1934-1937 Statistisches Reichsamt (1939/40) page 258*-259*.

Belgium

Central Government debt – long-term, short-term, foreign –and debt service: United Nations (1948) page 21 in millions of Francs on 31 December 1914-1938. Short term data includes medium term debt. Foreign debt data converted at current rates of exchange with the exception 1932 and 1934 francs loans converted at parity rates before the devaluation of October 1936. Intergovernmental debts are included throughout. Increase from 1924 to 1925 is partly due to certain war debts for which Belgium previously did not hold itself responsible being included in accordance with an agreement with the US. Debt service represents total expenditure against ordinay expenditure. Up to 1930 it includes ex service men's fund and pensions, which cannot be separated from the rest.Original source: Office Central de Statistique, Annuaire Statistique, Evolution des Finances de l'Etat, 1931-40, Banque Nationale, Bulletin d'Information et de Documentation.

Local Government debt: Statistisches Reichsamt (1939/40) on 31 December 1926-1937 in millions of Francs, data refers to provinces only.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 43, Jahreszahlen; 1934-1938 Statistisches Reichsamt (1939/40) page 298*-299*.

Bolivia

Central Government debt –long-term, short-term, foreign –and debt service: United Nations (1948) page 25 in millions of Bolivianos on 31 December 1914-1938. Starting from 1933 arreas of interest, which accumulated since 1927 are included, the total amount on 31 december is 3.2 millions, but cannot be separated on a yearly basis. Foreign debt data includes arreas of interest throughout. Original sources: Oficinia Nacional: Estadistica Financiera, Estadistica Boliviana. Direccion General de Estadistica: Extracto Estadistico. Ministeirio de Hacienda, Direccion General de Estadistica: Finanzas. Banco Central de Bolivia: Boletin.

Local Government debt: not available.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 424, Jahreszahlen; 1934-

1938 Statistisches Reichsamt (1939/40)page 258*-259*.

Brazil

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 27 in millions of Milreis on 31 December 1914-1938. The original short term debts figures included paper notes in circulation. These have been removed for better comparability. The debt service figure for 1933/34 is for 15 months ending 31 march 1934 and the debt service figyre for 1934 is for 9 months ending December 1934. The service of the foreign debt includes for 1931, 1934, 1937 and 1940-45 the portions not transferred but allocated to special accounts which forms part of the floating domestic debt. In september 1931, the payment on the foreign debt service was suspended. in march 1932 a plan for partial repayment of the service on the foreign debt was established for the period april 1934 to march 1938 and then suspended in 1937 again. Most of the foreign debt and a small part of the domestic debt are expressed in gold milreis. The gold milreis was a unit of account used starting from the 19th century to report certain items by the Brazilian government. The gold milreis figures have been converted into paper milreis at the official parity of 27 pence per gold milreis. The floating (short-term) debt has been converted into paper milreis at current rates of exchange. It is made up of promissory notes destined to liquidate frozen credits of foreign exporters in accordance with the commercial agreements between Brazilian government and foreign exporters. The decline from 1927 to 1928 is partly due to the fact that the French 5% 1908-09 Itapura-Corumba Railway Loan considered up to 1927 as being in gold francs was unilaterally converted into paper francs. Beginning with 1923, the funded (long term) domestic debt does not include obligations held by the Caixa de Amortizacao (1932: 32 millions paper milreis). Original sources: Contadoria Geral de la Republica: Balancos Gerais da Uniao (title varies slightly during period 1914-43), Anuario Estadistico do Brasil, Sir Otto Niemayer: report submitted to the Brazilian Government, 1931.

Local Government debt: Moody's (1936) between 1928 and 1934 in millions of paper milreis. Data is available only for external debt. The total debt figure is inferred by assuming that the proportion between internal and external debt is the same as the national one.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 426, Jahreszahlen; 1934-1938 Statistisches Reichsamt (1939/40)page 258*-259*.

Bulgaria

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page in millions of Leva on 31 December 1914-1926, 31 March 1928-1934, 31 December 1934-1938. Short term debt data include debts to national bank. Foreign debts converted at the appropriate parity for compatibility. Share of Ottoman debt owed by Bulgaria is not included throughout. Debt owed to Bulgaria due to property sequestrated and liquidated in the UK is also not included. Debt service includes reparations starting from 1928/29. During WWI, these payments were suspended and then resumed through agreements in 1920. 1925 and 1927 until further suspension in April 1932.

Local Government debt : Statistisches Reichsamt (1939/40) on 31 December 1926-1931 and 1935 and 31 March 1932-1934 in millions of Leva. Data includes Provinces and cities.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 43, Jahreszahlen; 1934-1938 Statistisches Reichsamt (1939/40) page 298*-299*.

Canada

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 34 in millions of Dollars on 31 March 1914-1938. Long term debt data, according to Canadian classification includes treasury bills and deposit certificates. Short-term floating debt consists of various demand liabilities. Total debt represents gross debt. Foreign debt is shown at parity. Original source: Public Accounts; Canada Year-Book.

Local Government debt: Statistisches Reichsamt (1939/40) between 1926 and 1936. The data includes provincial and municipal debt.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 377, Jahreszahlen; 1934-1938 Statistisches Reichsamt (1939/40)page 258*-259*.

Colombia

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 34 in millions of Pesos on 31 December 1927-1938. Domestic short term debt excludes state notes in circulation. The foreign debt is transformed in the national currency at parity $1=1 peso and £1=5 pesos. Redemption of foreign debt was suspended since 1932 and interest payments since 1935, however some partial

payments were made. Original Source: Informe Financiero de Contralor General, Anuario Estadistica, Boletin del departemento de Contraloria.

Local Government debt: Statistisches Reichsamt (1939/40) between 1926 and 1937 in millions of Pesos. The data includes the debt of municipalities and departments.

Gold reserves and notes in circulation: 1920-1933 Statistisches Reichsamt (1936a) page 453, Jahreszahlen; 1934-1938 Statistisches Reichsamt (1939/40)page 258*-259*.

Czechoslovakia

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 49 in millions of Kroner on 31 December 1918-1938. Long term domestic debt includes debt to national bank (state notes debt), except for for 1919-23. Part of the increase in the debt is due to the settlement and adjustment of pre-1914 and 1914-1918 war debts. The foreign debt 1920-1930 is converted into national currency at the rate ruling at the moment of inscription of the various loans in the debt ledger. Beginning with 1934, the debt in foreign currency has been converted at the rates of exchange on respective dates. Some of the original debt service figures do not include commissions, I have added them to improve comparability. The reduction in amortization and interest in foreign debts since 1931 was due to the Hoover Moratorium of June 1931 and the Lausanne conference of 1932. From 1933, the regular redemption of domestic debt was suspended, but bonds were accepted for payment of arreas of taxes and purchases of bonds were effected by the state. Original sources: Closed Accounts, Office National Statistique, Renseignements Statistique. Ministry of Finance, Dr. J. Dolansky, Vyklad k Rozpoctu Na Rok 1947 (budget Expose' 1947).

Local Government debt: No continuous series is available. A single data point for 1933 is present in Statistisches Reichsamt (1939/40).

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 293, Jahreszahlen; 1934-1938 Statistisches Reichsamt (1939/40)page 258*-259*.

Denmark

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 51 in millions of Kroner on 31 March 1914-1938. In 1933/34, the loss on exchange in repayment of the foreign debt is included with "interest". The short-term debt includes treasury bills and, up to 1931, also debts from unpaid interest. It does not include overdrafts on ther current account of the ministry of finance with the

central bank and other banks which are normally more than offset by assets with those banks. Debt towards central bank is available but has not inculded for compatibility with other countries where it is reported on net. The foreign debt is shown at legal parity, except for some French loans. Beginning in 1925/26, interest payments as published by official accounts are offset by interest received from capital invested in real estate, plan equipment and by a percentage invested in capital for depreciation. The net balance is added or deducted from current receipts. Data on total interest received is available, but negligible. I also use real cost of redemption rather than the nominal one. Original Sources: Statsregnskab (closed accounts), Statistik Aarbog, Danske Staatslaan.

Local Government debt: Statistisches Reichsamt (1939/40) on 31 March between 1926 and 1938 in millions of Kroner. The data includes the debt of municipalities and departments.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 62, Jahreszahlen; 1934-1938 Statistisches Reichsamt (1939/40)page 258*-259*.

Finland

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 60 in millions of Finnish Marks on 31 December 1916-1938. Data excludes debts repurchased and held by public debt administration. Debts are shown at book value. For 1914-1931, the debt service is total expenditure charged against ordinary revenue (receipts exceeding proceeds from loans). For 1932-1945, the expenditure is charged against current receipts. Foreign interest payments include exchange losses. For 1932-1945, redemption payments are charged against capital receipts, which include proceeds from loans. Amounts therefore include conversions. Original Sources: Accounts, Bureau Central de Statistique, Annuaire Statistique de Finlande, Recueil de Statistique, Communication from Bank of Finland, Institute for Economic Research.

Local Government debt : Statistisches Reichsamt (1939/40) on 31 December between 1926 and 1936 in millions of Finnish Marks. The data includes the debt of municipalities and other local communities.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 62, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

France

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 64 in millions of Francs on 31 December 1917-1919 and 1922-38, 31 May 1920 and 31 March 1921. Debt figures up to December 1937 are in comptes generaux, yearly data, figures after 1937 are not directly comparable to earlier ones. Long term debt also includes medium term debt, mainly bonds of more than two years maturity at time of issuance (which are long term debt according to the UN classification used in their 1948 volume). For 1914-1935,short term debt includes advances from the cental bank, while for 36/37these are excluded and re-included starting from 1938. Foreign debt was converted in Francs at the exchange rate at the date in question. The foreign debt was excluded from official statements from 31 march 1932 to 31 december 37. Starting from 1938, data excludes excludes interallied debts from WWI. The debt service data for 1920/21 are figures from 1920,. 1921/22 figures from 1921, 1929/30 from1929, for 1929 last 9 months only. 1927-32 are provisional figures. Original Sources: Closed Accounts (Comptes Generaux), Ministe're des Finances, Dette Publique (Situation Mensuelles), Inventaire de la Situation financie're (1913-1946).

Local Government debt data: Statistisches Reichsamt (1939/40) on 31 March between 1926 and 1930 in millions of Francs. The data includes the debt of municipalities and departments. The figures are very small compared to the central government debt (around 3% in 1930). For this reason they are assumed to be negligible for the empirical analysis.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 95, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Germany

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 67 in millions of Reichsmarks on 31 March 1914-1938. Between 1913/14 and 1922/23 most of the debt (almost entirely internal in nature) was wiped out by the hyperinflation and the redemption of the paper mark Treasury bills held by the Reichsbank by payment in new Rentenmarks (1 rentenmark= 1 trillion paper marks). The increase from 1925 to 1926 was due to the fact that most "pre-stablisation" debts had been converted into the new "loan liquidation debt" which was shown for the first time in the public debt statement of 1 March 1926, this amounted to 5,500 millions RM. For 1919/20 to 1922/23, the long term includes debts taken over from the states (largely railway). For the same period, miscellaneous obligations and guarantees in the form of treasury bills are

included. For 1932/33 to 1944 tax payment certificates are included. The foreign debt is shown at par until 31 march 1933. From march 1934 loans issued in US Dollar, Sterling an Swedish Kroner have been converted at the mean exchange rate of the respective years. The decrease from 1933 to 1934 is chiefly the result of the change in the conversion rates. The further gradual decline is due not only to actual transfers to the creditors abroad but has resulted also from amounts in marks deposited in favour of foreign creditors at the Reichsbank but not transferred due to the lack of foreign exchange, which have been deducted from the outstanding debt. These amounted on 30 sept. 1944 to Reichsmarks 150.4 million. Original sources: Closed Accounts, Reichs-und Staatsanzeiger, Statistiches Reichsamt, Statistiches Jahresbuch, Wirtschaft und Statistik.

Local Government debt: Statistisches Reichsamt (1936b) between 1928 and 1934 Statistisches Reichsamt (1938) in millions of Reichsmarks; 1935-1938 Statistisches Reichsamt (1938). The data includes the debt of States, Municipalities and Free Cities.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 16-17, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40) page 258*-259*.

Greece

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 71 in millions of drachmae on 31 December for 1913-1918 and 31 March for 1913-1938. The foreign devt is shown in gold drachmae at pre 1914 parity for 1913-27. From 1928, it is shown at the new parity. The debt service is the total interest due on foreign loans. The non-transfered portions are set off on the receipt side of the budget account as loans guaranteed by creditors. Redemptions include expenditure from loan receipts. Original sources: Annuaire Statistique de la Grece, Bulletin Mensuel Statistique.

Local Government debt: No continuous series is available.

Gold reserves and notes in circulation: No continuous series is available.

Hungary

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 67 in millions of Peng on 30 June 1924-1938. Beginning with 30 june 1932 the monthly reports on the financial conditions of Hungary include ceratin items not shown in the original UN figures. These additional amounts are available and I have included them together with administrative liabilities. The foreign debt is shown at official

rates of exchange on the corresponding dates. The decrease in 1932 has been due in part to the depreciation of the Sterling and US Dollar. The debt service figures do not include the amounts of debt service paid directly by the public undertakings. For 1926/27 and 1927/28 administrative expensive have been included. Since december 1931, the foreign debt service has been partly suspended and the Peng equivalents of the untransferred portion were paid into blocked accounts from where large amounts re-borrowed by the Hungarian government against treasury bills. Original sources: Closed accounts, Monthly Statements on Financial Conditions in Hungary.

Local Government debt: No continuous series is available. A single data point for 1931 is present in Statistisches Reichsamt (1939/40).

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 304. Jahreszahlen. Reserves only: 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Ireland

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 87 in millions of Pounds on 31 March 1923-1938. In addition to amounts recorded in the UN volume there is a liability under the housing act of 1932 and also a liability under the damage of property act amounting on 31 March 1945 to £9.4 and £4.3 million respectively. The latter liability on which an annuity of £250,000 is payable for a sixty year period commencing in 1926 has been included since 1944/45 in the official debt statements. The debt redemption consists of net amortization payments out of ordinary revenue. Original Sources: Eire Finance accounts; Ireland Statistical Abstract.

Local Government debt data: Statistisches Reichsamt (1939/40) on 31 March between 1926 and 1937 in millions of Pounds. The data includes the debt of all local bodies.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 140. Jahreszahlen. Reserves only: 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Italy

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 89 in millions of Lire on 30 June 1914-1938. The foreign debt was converted at old parity rates. The original foreign debt figures exclude the war debt 1914-1918, consisting chiefly of obligations to the governments of the UK and the US, I have included these to improve comparability. I have also included interest payments on the war debt of 1914-1918. Redemptions include conversions. Original sources: Rendiconto

Generale, Annuario Statistico Italiano, Compendio Statistico Italiano.

Local Government debt: The figures for 1926 and 1935 are from Statistisches Reichsamt (1939/40). The data in between is estimated using the shares of local and central government debt in Francese and Pace (2008) and the central government debt in United Nations (1948). All data is in millions of Lire and covers all local public bodies.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 144, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Japan

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 92 in millions of Yen on 31 March 1914-1938. The figures include the debt incurred for the general as well as the special accounts. The figures presented under "other borrowings" in the UN volume represent "loans at various rates of interest" which could not be subdivided into domestic and foreign issues. I have added them to the domestic debt. The short term debt includes special exchequer notes, treasury notes, rice purchase notes and silk-purchase notes. The foreign debt was converted into yen at the gold parity rates (£1= 9.763 yen, $1= 2.006 yen, 1 French Franc = 0.387 yen). Original sources: Department of Finance, financial and economic annual of Japan, resume' statistique de l'empire du Japon, Bank of Japan, Economic Statistics of Japan, Oriental Economist, Supreme commander for the allied powers, Japanese economic statistics.

Local Government debt: Statistisches Reichsamt (1939/40) on 31 March between 1926 and 1937 in millions of Yen.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 37, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Netherlands

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 90 in millions of Guilders on 31 December 1914-1938. Total domestic debt does not include advances by the national bank guaranteed by the government against assets in Reichsmarks amounting to Gulden 4,500 million on 31 december 1944. The foreign debt, when present, is included in the amounts shown under "long term domestic debt". In 1922 two loans of Florins 125 million and in 1924 another loan of florins 100 million were issued in America. These loans were redeemed in 1932 and 1929 respectively. Total interest payments defrayed from ordinary receipts and up

to 1933 inclusive also expenditure from Loan Fund. Receipts of the loan fund which was abolished in 1934 consisted chiefly of taxes additional to certain excise funds. The domestic redemption represents expenditure charged against ordinary revenue only. Certain extraordinary payments were made during 1920-1944. Original sources: Jaarcifers voor Nederland, Maandschrift van het Centraal Bureau voor de Statistiek.

Local Government debt: Statistisches Reichsamt (1939/40) on 1 January between 1927 and 1938 in millions of Guilder. The data covers local communities and provinces.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 185, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Norway

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 106 in millions of Kroner on 30 June 1914-1938. The long term debt figures after June 1932 do not include the loan from the invalidity fund amounting to kroner 4.5 million on the above mentioned date. The figures include the "unpaid capital by the state-guaranteed banks". From 30 June 1933, the item is not included with the debt proper but shown separately as a state liability in the official accounts. The foreign debt is shown at parity rates (£1=18.16 Kroner, 1 Franc=0.72 Kroner, $1 =3.73 Kroner, 1 Swedish Kroner= 1 Norwegian Kroner, 1 Swiss Franc= 0.72 Kroner with the exception of certain loans in Pounds converted at the rate £1=18.12, 18.16 or 18.18 Kroner and Swedish Kroner converted at the rate of 1 Swedish Kroner=1.02575 Norwegian Krone. Interest payments include commissions and other expenses. Redemption expenditure is charged against capital receipts (including proceeds from loans). Original sources: Closed Accounts; Statistique Officielle de la Norve'ge, Seire VIII; Statistical Year-books of Norway.

Local Government debt: Statistisches Reichsamt (1939/40) on 30 June between 1926 and 1936 in millions of Kroner. The data covers municipalities and prefectures.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 197, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Peru

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 113 in millions of Soles on 31 December 1918-1938. Non comparable data is available for the period 1914-1917. The foreign debt was converted

into Soles at parity rates. The increase in foreign debt in 1930 and 1931 was chiefly due to the adoption of higher converson rates. As interest payments on the foreign debt were suspended in May 1931, arreas of interest were added to the outstanding capital. The service on the foreign debt was completely suspendeed with the exception of the stetling 7.5% guano loan. Interest payments on the foreign debt were partially resumed in 1937. Original sources: Balance y Cuenta de la Republica, Extracto Estadistico del Peru.

Local Government debt: figures are available for external debt only, between 1927 and 1933 from Werhahn (1937).

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 482, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*. Data also includes foreign exchange reserves.

Poland

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 116 in millions of Zloty on 31 March 1919-1938. Short term debts for the period 1919-23 are in Polish marks and consist mostly of treasury bonds and loans by the central bank in form of large advances in paper money to the government. For the period 1924-39, short-term debts consist of treasury notes, non-interest bearing advances from the Bank of Poland and the remainder of bonds and notes matured. The foreign debt for the period 1919-23 is shown in Zloty at the rate of 5.183 zloty=$1. For 1924-1939, the foreign debt is shown at the rates of exchange on the respective dates with the exception of the dollar issue of the 7% stabilisation loan of 1927 and the debt taken over from the former Austrian monarchy, which have been converted at par. Original sources: Budgets, Closed Accounts, Annuaire Statistique de la Republique Polonaise, the Statistical Bulletin of the Ministry of Finance, the Bulletin of the Bank of Poland, Central Statistical Office, Statistical News.

Local Government debt: Statistisches Reichsamt (1939/40) on 31 March between 1926 and 1938 in millions of Zloty. The data covers all local public bodies.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 197. Jahreszahlen. Reserves only: 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Romania

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 124 in millions of Lei on 31 March 1919-1938 except for 1923-

31 on March 31. Long term debts since 1925 include consolidated debts owed to the central bank. or 1920-24 these were included in the short term debts. From 1933 short term debts include temporary advances from the central bank. For 1914 the foreign debts converted at the pre-1914 parity; for 1929-31 is is calculated at new parity rates. For 1933 the debt in sterling is converted at the rate £1=560 lei and $1=110 lei at at stabilization rates for other currencies. From 1935, war debts are excluded from the figures, I reintroduce them to improve comparability. Redemption of both foreign and domestic debt was suspended in 1933. Original sources: Budgets, Central Statistical Institute, Statistical Year-books, National Bank, Bulletin d'Information et de Documentation.

Local Government debt: Not available.

Gold reserves: Not available.

Sweden

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 133 in millions of Kroner on 31 December 1914-1922 and 30 June 1923-28. The original figures of domestic long-term includes foreign debt starting from 30 June 1923. I have separated the two using foreign debt figures on 31 December of the same year. The foreign debt was converted into kroner at parity and consists solely of bonds in the hands of creditors resident abroad. The fall in foreign debt is partially due to repayments and repurchases by Swedish residents. The debt service represents total budgetary expenditure. Original sources: Closed Accounts, Year-books of the National Debt Office (Riksgaeldskontoret), Statistical Year-books of Sweden.

Local Government debt data: Statistisches Reichsamt (1939/40) on 31 December between 1926 and 1935 in millions of Kroner. The data covers municipalities and provinces.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 260, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Switzerland

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 137 in millions of Francs on 31 December 1914-1938. The original figures for the public debt are those of the confederation, excluding the debt of the Cantons and the federal railways debt. I have included the railways debt. The debt service figures do not include railway debt. Administrative costs are included in redemptions up to 1923 and in interest payments from 1924. The debt service expenditure is charged

against ordinary revenues (excluding expenditure from loan proceeds). Original source: Comptes d'Etat, Statistical Year-books of Switzerland.

Local Government debt: Statistisches Reichsamt (1936b) between 1925 and 1935 in millions of Francs. The data covers the cantons and municipalities.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 272, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

United Kingdom

Central Government debt – long-term, short-term, foreign – and debt service: United Nations (1948) page 147 in millions of Pounds on 31 March 1914-1938. The aggregate liabilities of the state do not include the funding loan and victory bonds tendered for death duties but not yet cancelled. The foreign debt is shown at par. The debt service includes not only appropration from revenue but also interest payments on national savings certificates paid in excess of the provision in the permanent debt change and interest met from receipts under section 1 (5) (b) of the defence loan act, 1937. Payments on the war debt of 1914-1918 have been suspended completely since 1934. Original source: Finance Accounts of the United Kingdom.

Local Government debt: Statistisches Reichsamt (1939/40) on 31 March between 1926 and 1936 in millions of Pounds. The data covers England, Wales and Northern Ireland, but not Scotland.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 123, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Uruguay

Central Government debt – long-term, short-term, foreign – and debt service United Nations (1948) page 154 in millions of Pesos on 31 December 1914-1938. The domestic debt includes the so-called "international debt", consisting of two Brazilian issues, which are payable in Uruguayan Pesos and are held in Uruguay. No information is available for short-term debts between 1914 and 1928. The foreign debt was converted at old parity rates (4.7 Pesos=£1), 0.966 Pesos =$1, 0.1865 Pesos=1 gold Franc). In 1915 the redemption of the the foreign debt was suspended in agreement with the bondholders. In 1916, it was agreed to prolong the suspension until one year after the war. Redemption of the foreign debt was again suspended on 20 January 1931 and resumed partially on 1 January 1937 and completely on 1 January 1939. Domestic debt payments were sus-

pended on 1 November 1932 and resumed on 27 February 1937. I have added/substracted exchange losses to the debt service in order to improve comparability. Original sources: Deuda Publica Nacional, Anuario Estadistico.

Local Government debt: the figures from 1928 to 1933 are from Moody's (1935) and from 1936 to 1928 from Statistisches Reichsamt (1939/40). The debt is recorded on 31 December in millions of Pesos.

Gold reserves: 1920-1933 Statistisches Reichsamt (1936a) page 490, Jahreszahlen; 1934-1938, Statistisches Reichsamt (1939/40)page 258*-259*.

Yugoslavia

Central Government debt – long-term, short-term, foreign – and debt service: not available.

Local Government debt: Statistisches Reichsamt (1939/40) on 31 December for 1930, 1933 and 1936 from .

Gold reserves: not available.

3.B.3 Alternative sources

Moody's manuals offer an excellent opportunity to cross-check the data collected from other sources. While the manuals do not generally offer time series that are as complete and systematic as the German sources and the League of Nations, they contain a huge amount o information that the agency used for its own ratings (Gaillard, 2012) and was thus very careful in collecting. The local debt debt has been carefully cross-checked whenever possible thanks to the sometimes substantial overlap between different sources. The picture is that of general agreement. The general rule in case of disagreements between the sources has been to prefer the later source to account for revisions.

Chapter 4

Paper 2: Fiscal Capacity, Tax Composition and the (In-)Stability of Government Revenues in the Interwar Period

4.1 Introduction

> *Fiscal policy is both simple and incredibly complex to evaluate between the wars: apparently simple to comprehend, because the goals appear straightforward [...]. In practice, however, evaluation is incredibly complicated, as the period witnessed a complex of enormous pressures for expenditure growth, a budget now significantly enlarged from pre-war and highly cyclically sensitive, and all of this within the context of considerable macroeconomic instability and the pressures for the Keynesian solution which, importantly, predate the 1929 downturn.*
>
> Middleton (2013)

As Roger Middleton argues in the quotation above, fiscal policy in the interwar period was a complicated affair. The author highlights this with reference to the United Kingdom, but other countries faced even more formidable challenges, due to significantly greater economic instability combined with younger, less centralized and less fiscally endowed states. Both public revenues and expenditures were extremely volatile in the interwar period and this contributed to overall uncertainty in the economic and political spheres.

Thus, studying this unique era, apart from the interest in the event itself, allows me to assess the behavior of governments under extreme economic circumstances, which put their very existence into question, as demonstrated by the numerous revolutions and regime changes, which took place in the early 1930s.

This paper investigates the determinants of the volatility of fiscal aggregates and, consequently, of fiscal policy in the interwar period by engaging with the following questions: 1) How did fiscal capacity affect the volatility of tax revenues and government financing in the interwar period? 2) Through which channels did this effect occur? 3) How did fiscal capacity influence countries' ability to borrow and thus run fiscal deficits? I tackle these questions using newly collected data for a large panel of developing and developed countries (19 to 35, depending on the specification) .

I carry out the empirical analysis that is the centerpiece of this paper in three steps. First, I show that the fiscal capacity of countries – as proxied alternatively by overall tax revenue over GDP and income tax revenue over GDP[33] – played a major role in reducing the volatility of government financing. Second, I show that the smoothing effect of fiscal capacity worked principally by guaranteeing countries an easier access to borrowing. Specifically, I show that high capacity countries had higher initial debt stocks, were able to borrow more compared to low capacity countries in the interwar years, and also had lower sovereign bond yield spreads, which allowed them to borrow more cheaply. Third, by focusing on the composition rather than just the magnitude of tax revenues, I provide evidence that this effect was due to fiscal capacity signaling higher institutional quality, rather than simply a higher present value of future tax receipts.

Naturally, this type of exercise is potentially vulnerable to many sources of bias and measurement error.[34] I deal with the potential endogeneity of fiscal institutions and the confounding effects of temporary changes in tax policy with two strategies. The first is holding the structure of fiscal systems stable by using 1914-1926 average fiscal indicators to study changes in government revenues between 1927 and 1938. The second is an instrumental variable approach.

I employ two instruments for fiscal capacity: 1) the number of years spent by countries in major external conflicts between 1816 and 1913, as reconstructed by Dincecco and

[33]These are two commonly used fiscal capacity indicators. See for example O'Brien (2011) and Besley and Persson (2009)

[34]For example, if countries with more volatile revenues decided to invest more in fiscal capacity to mitigate this, the effect of the latter would be underestimated in the regressions. Rodrik (1998) discusses the issue of the co-determinateness of volatility and government size in the context of estimating the effect of openness on government size. Conversely, if the revenue volatility made investments in fiscal capacity more difficult, the impact of fiscal capacity would be overestimated.

Prado (2012), 2) the number of natural disasters to hit a country between 1900 and 1926, which I calculate based on data from the EM-DAT dataset of the Centre for the Epidemiology of Disasters (CRED, 2017). The choice of these variables has historical, theoretical and empirical foundations that are discussed in detail below. In short: external conflicts facilitate the extension of the state's franchise because they lead to an exogenous increase in the demand for a public good – national defense – which needs to be financed through taxation. Indeed, the historical record shows that warfare encouraged reforms, which had far reaching consequences for the fiscal development of countries. Natural disasters, instead, are a counter-force to the concentration of fiscal resources in national governments. This is because of their predominantly small scale, which spurs the creation of targeted local, rather than central, institutions and revenue streams to deal with both their prevention and consequences.

The paper speaks to four main strands of literature. First, it addresses the economic history literature on policy reactions to the Great Depression. Amongst many others, Temin (1989) and Eichengreen (1992) have pointed out that these were either misguided – in core countries – or extremely limited – in the periphery. Eichengreen maintained that this was due to the constraints imposed by the dysfunctional interwar Gold Standard, in conjunction with weak international cooperation and an inadequate conceptual framework based on balancing the budget. Temin similarly claimed that the Gold Standard was the key mechanism for the diffusion and severity of the slump. I argue that the pre-existing structure of fiscal systems also severely constrained the policy responses of countries that saw their tax revenues collapse and financial markets dry up. Thus, besides the Gold Standard straitjacket invoked by the literature, countries faced additional and more-deeply rooted constraints. This is also illustrated in my first paper (Chapter 3), in which I show that the collapse in public revenue experienced by some countries played a key role in the debt crisis of the early 1930s: countries that experienced larger decreases in their revenues were more likely to default and to undergo larger defaults.

Second, this paper contributes to the literature on the cyclicality of fiscal policy and the literature on the determinants and impact of macroeconomic volatility. It is now a well-established stylized fact that in the post-World War II era industrialized economies have consistently run countercyclical or acyclical fiscal policies, while developing countries have followed procyclical ones, which have presumably contributed to their macroeconomic volatility.[35] High fiscal policy volatility is not only related to macroeconomic volatility,

[35]See Gavin and Perotti (1997), Kaminsky, Reinhart, and Vegh (2004), Talvi and Vegh (2005), Mendoza and Oviedo (2006), Ilzetzki and Vegh (2008), Frankel, Vegh, and Vuletin (2013) and Vegh and Vuletin

but also to lower growth. Fatás and Mihov (2013) provide empirical evidence for this link, while Robinson, Torvik, and Verdier (2017) illustrate a potential political economy channel for it, which ties budget volatility to a lower efficiency in public policy. A further stylized fact for the post-WWII era is that countries with larger governments tend to have less volatile economies. This has been attributed to the combination of automatic stabilizers[36] and a composition effect (Galí, 1994; Fatás and Mihov, 2001; Andrés, Doménech, and Fatás, 2008). The argument for the latter is that the public sector is more stable than the private sector, therefore countries with larger public sectors will be less volatile overall.

I add an historical dimension to this literature by showing that both government revenue and expenditure were more volatile than GDP on average in the Great Depression years. Thus, the historical record, as well as the experience of developing countries today (Mendoza and Oviedo, 2006), demonstrate that a lower volatility in the public sector compared to the private sector should not be taken for granted. The paper's findings also support the the notion that institutionally-determined credit constraints in countries with weak and small governments can play an important role in fiscal policy pro-cyclicality and volatility. Consequently, the ability of stronger governments to run more counter-cyclical and less volatile fiscal policies might help explain the negative correlation between government size and macroeconomic volatility and, as a consequence, also lower growth.

Third, my findings shed further light on the far-reaching impact of fiscal institutions on economic outcomes. The literature linking fiscal capacity to long-term economic development both theoretically and empirically is now an established and burgeoning field. I review this literature in detail in the introductory portion of this Dissertation (Section 2.1). Even though there is a consensus that fiscal development is an important determinant of the ability to borrow (North and Weingast, 1989; Bordo and White, 1991; Dincecco, 2009; O'Brien, 2011), there are no empirical studies linking fiscal capacity directly to cyclical economic outcomes and fiscal policies. This paper helps fill this gap.

The final contribution of the paper is to provide new data on the fiscal history of the interwar period. At the time, the fiscal development of countries was still very limited. Levels of taxation in rich countries were comparable to those of poor countries today, while poor countries already lagged behind in their fiscal capacity. The extreme economic downturn and widespread collapse in public revenues caused by the Great Depression forced a broad spectrum of countries to reconsider their taxing strategies, and for some

(2015), amongst others.

[36] See McKay and Reis (2016) for a recent study of automatic stabilizers in the US which finds that they had a limited role in dampening the business cycle in the last two decades.

nations the early 1930s represent the beginning of far-reaching changes in taxation and in the role of governments in the economy. In the United States, for example, the expansion of income taxation and of Federal spending programmes in the 1930s ushered in a new era (Wallis, 2000; Fishback and Wallis, 2013). This is also true at the sub-national level: Gillitzer (2017) shows that US states that were hit more severely than average by the Depression were more likely to introduce new taxes compared to states hit by smaller slumps. Argentina provides another illustration. The country introduced its very first income tax in 1932 as response to the Depression (Alhadeff, 1985). Furthermore, the share of countries with withholding doubled during the interwar period opening the way for the tax systems we have today (Besley and Persson, 2014).

More generally, using newly-assembled data, I document that the rapid increase in fiscal capacity in Europe and North America witnessed over the course of the 20th century was already under way in the interwar period, and that it was particularly rapid in the aftermath of the Depression. Events such as WWII and the consolidation of welfare states in the War's aftermath certainly contributed to the patterns of taxation we see today, but divergent paths were already visible in earlier periods. The data also includes local governments; therefore, this paper is a primer in terms of local level tax data. However, further work is necessary to increase the coverage on local governments – in the interwar period, as in many other historical contexts – in order to fully grasp changes in taxation patterns (Hoffman, 2015; Dincecco, 2015).

The rest of the paper is structured as follows. In Section 4.2, I discuss how revenue volatility can affect policy and the action of states more generally. In Section 4.3, I briefly discuss the data, with particular emphasis to the evolution of fiscal capacity over the interwar period. Section 4.4 illustrates the econometric strategy to investigate the link between fiscal capacity and government financing smoothing. Section 4.5 discusses the results of the estimation and their quantitative importance, while Section 4.6 explores the borrowing channel through which fiscal capacity affected government financing. Section 4.7 concludes.

4.2 The impact of revenue volatility

Figure 4.1 illustrates annual percentage changes in central government tax revenues, financing (tax revenues+non-tax revenues+long-term borrowing) and expenditure in absolute terms and as a share of GDP in the interwar period for the countries in my sample. These are Argentina, Australia, Austria, Belgium, Bolivia, Brazil, Bulgaria,

Canada, Chile, Colombia, Czechoslovakia, Denmark, Egypt, Estonia, Finland, France, Germany, Greece, Hungary, India, Indonesia, Ireland, Italy, Japan, Latvia, Lithuania, the Netherlands, New Zealand, Norway, Peru, Poland, Romania, South Africa, Spain, Sweden, Switzerland, Turkey, the United Kingdom, the United States, Uruguay and Yugoslavia with various degrees of temporal coverage between 1927 and 1938.[37]

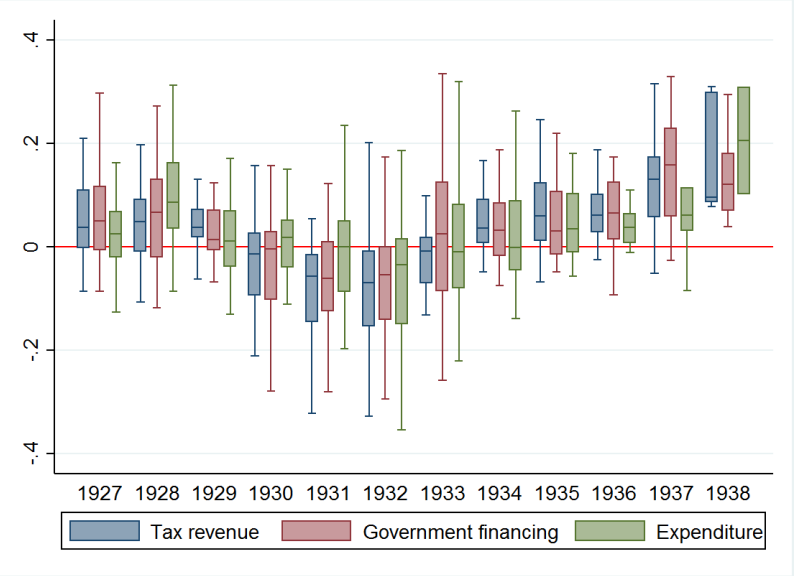
The dispersion of the variables is very large and – as panel (b) demonstrates – can only be partially attributed to changes in economic activity. In fact, as mentioned above, both government revenue and expenditure were more volatile than GDP in this period. Although it is not immediately apparent from the charts, the analysis below shows that tax revenues moved very closely with economic activity. Government financing, instead, was less responsive to changes in output thanks to the contribution of non-tax revenues, which were essential in smoothing government expenditure. I show below that fiscal capacity was a key determinant of this smoothing effect because it provided an easier access to borrowing.

The government financing variable is also interesting because it represents how statistical offices in the interwar period classified public revenues. Moreover, it is a measure of the planned and budgeted part of government expenditure and fiscal policy more generally, which made up the vast majority (96%) of public budgets. Although short-term borrowing was used to make up for budget short-falls, the financing variable, and long-term borrowing in particular, are the portions of public intakes that are most closely related to fiscal capacity and the quality of institutions more generally.

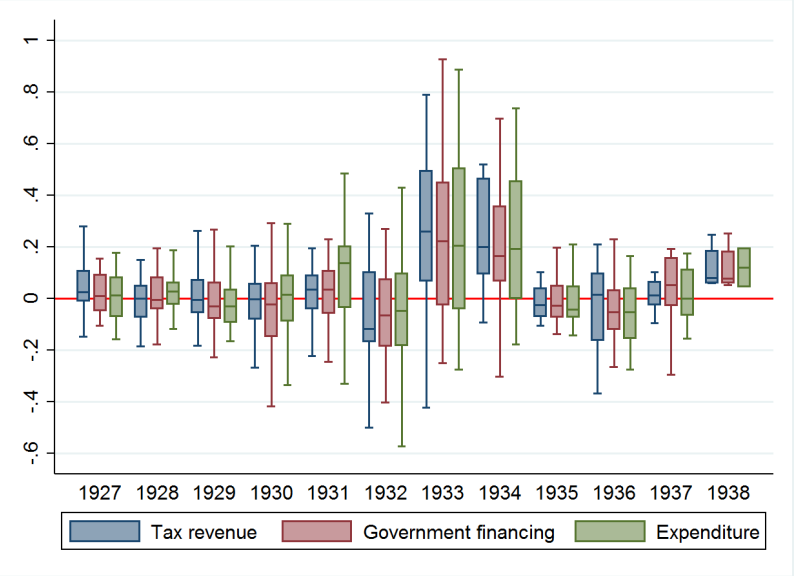
4.2.1 Fiscal policy: the interwar experience

Given this backdrop of revenue instability, how was fiscal policy conducted in the interwar period? Fiscal deficits, when at all present, tended to be very small, especially in comparison to the loss of GDP (Almunia, Bénétrix, Eichengreen, O'Rourke, Rua, Tenreyro, and Perri, 2010). For example, in the US the deficit was less than 6% of GDP in 1933. By comparison, in 2009 it was 10%. At the same time, the cumulative contraction of real GDP per capita between 2007 and 2009 was less than one sixth of the Great Depression one, around 5% compared to 31%. Additionally, deficits in early 1930s US were mostly the unexpected product of mistakes in budgetary planning, rather than a form of proto-Keynesianism, which developed only late in the decade (De Long, 1998). Even in Germany, a country

[37] Some of these countries are later left out of the analysis due to lack of data on the co-variates. Greece, instead is left out of this because it experienced some very dramatic changes in revenue which cloud the overall picture.



(a) Absolute numbers



(b) As a share of GDP

Figure 4.1: Yearly percentage changes in central tax revenues, government financing and expenditure, 1927-38

Government financing is composite, which includes tax revenues, revenues from publicly owned enterprises and capital, other unclassified revenues and long term (over 1 year maturity) borrowing and represented 96% of total government financing on average in the interwar period. Source: Statistisches Reichsamt for the revenue data and Klasing and Milionis (2014) for the nominal non-PPP-adjusted GDP data. See Appendix 4.B for details.

that is conventionally seen as having embraced Keynesianism ante-litteram, the recovery of the 1930s was not driven by fiscal policy (Ritschl, 2002a); a similar story applies to Sweden (Schön, 2007).[38]

The extremely limited use of fiscal (and monetary) policy has led researchers to ar-

[38] As cited by Almunia, Bénétrix, Eichengreen, O'Rourke, Rua, Tenreyro, and Perri (2010)).

gue that governments' responses to the Great Depression were gravely inadequate and potentially aggravated the slump. In Barry Eichengreen's classic book "Golden Fetters" (Eichengreen, 1992), the author highlights the lack of international cooperation – fuelled by mutual suspicion – and the dominant conceptual framework – underpinned by the balanced budget ideology – as two key determinants of the weak policy responses to the Depression. Indeed, even for those potentially well disposed towards Keynesianism, the dogma of the balanced budget was hard to displace. The smoothing of the business cycle through fiscal and monetary policy was a radical notion even among economists in the Labour camp in the UK – see for example (Dalton, 1934) – or the Communists in France (Eichengreen, 1992). Eichengreen furthermore identifies the dysfunctional interwar Gold Standard as the institutional straitjacket that limited countries manoeuvring space on both the monetary and fiscal fronts. According to the author, the unwillingness to let go of the gold anchor, seen by many as the last connection to the successful pre-WWI monetary order, constrained expansionary policies and piled further deflationary pressures and austerity on ailing countries. Temin (1989) similarly argued that the Gold Standard-imposed limitations to maneuvering space for policy were the fundamental channel for the spread of the Depression.

But how much room for manouver would countries have possessed had they been free from the *golden fetters*? What constraints, apart from Gold Standard membership, did they face? On the fiscal side, many countries probably had limited possibilities to act for a number of reasons. First, governments in the interwar period were small. Leaving aside the issue of fiscal capacity and borrowing constraints for a moment, the simple fact that states were modestly-sized relative to the economy made large scale fiscal stimuli practically impossible to implement. Second, sources of finance for governments shrunk very quickly during the Depression. After 1929, international financial markets, which had been bolstered by an outburst of US foreign lending after WWI, essentially dried up. Many countries that had experienced substantial capital inflows during the second half of the 1920s experienced dramatic reversals and capital flight. On top of this, domestic financial markets were underdeveloped in many countries and the legacy of the 1920s hyperinflations and the post-'29 collapse in asset prices greatly reduced the wealth governments could tap for domestic borrowing. Borrowing from central banks was certainly an option for countries not on gold. However, it is difficult to imagine that the scale of the loans some countries would have needed to counterbalance the funding shortfall through money printing alone would not have had major economic consequences in terms of inflation and/or further

capital flight.

I argue that, in addition to these factors, a low level of development of the fiscal system was a major constraint on countries' ability to smooth government financing, or – at a minimum – limit the procyclicality of public expenditure through borrowing. The argument is supported by the econometric analysis below. In Section 4.5.4, I discuss the specific examples of the United States and Argentina to further illustrate how a low level of fiscal development translated into damagingly pro-cyclical fiscal policy in the interwar period.

4.2.2 Beyond fiscal policy: revenue volatility and the action of states

Apart from contributing to procyclical fiscal policies, public revenue volatility can have further negative repercussions through four main channels. First, if revenue volatility contributes to macroeconomic volatility it can negatively impact economic growth (see Loyaza, Ranciére, Servén, and Ventura (2007) for an overview of the link between volatility and growth).[39] Empirical evidence for the post-WWII period highlights a link between the size of governments and economic volatility: countries and regions with larger governments consistently display a lower volatility of output (Galí, 1994; Fatás and Mihov, 2001; Andrés, Doménech, and Fatás, 2008). Some studies take this as a starting point to argue that countries facing more external shocks due to their openness will choose to have a larger government sector as a form of self-insurance (Rodrik, 1998; Epifani and Gancia, 2009). However, the channels through which governments may actually stabilize the economy are not yet clear. Researchers have argued that automatic stabilizers and a simple composition effect might both contribute to the smoothing of output.[40] Automatic stabilizers were extremely limited in geographical diffusion and scope in the interwar period, however, and their role in dampening business cycle fluctuations is unclear even in modern economies; see for example McKay and Reis (2016) on the US. The composition effect depends on the assumption that the public sector is less volatile than the private sector and, thus,

[39] Bleaney, Gemmell, and Greenway (1995) and Ebeke and Ehrhart (2011) offer interesting insights by focusing on sub-Saharan Afica. Both studies find adverse effects of revenue instability on the volatility of government investment and expenditure, and on the level of public investment.

[40] The mechanism through which government size reduces macroeconomic volatility proposed in the recent literature (Andrés, Doménech, and Fatás, 2008) is quite different in spirit to how the smoothing effect of automatic stabilizers in the traditional Keynesian framework. The latter posits that taxes react more than proportionally to income shocks so that disposable income, and therefore consumption of credit-constrained consumers, are smoother than income (De Long and Summers, 1986). This basic mechanism can be extended to government transfers and general government spending. In Andrés et al, however, the requirements of Real Business Cycle (RBC) general equilibrium modeling lead to the outcome that governments smooths consumptions because higher taxes mean that consumers have less disposable income to begin with, so that when a productivity shock hits, the fall in disposable income is smaller in relative terms.

a stabilizing force in the economy. This, however, is based on the limited experience of developed countries in the recent past. In modern developing countries and in the interwar period, governments were certainly not a stabilizing force on average, given that both their revenues and expenditures were more volatile than GDP. The volatility of government financing can thus play an important role in determining to what extent governments reduce or increase macroeconomic volatility.

Second, as shown empirically by Fatás and Mihov (2013), countries with more volatile fiscal policies have substantially lower long-term growth. Robinson, Torvik, and Verdier (2017) propose a political economy mechanism though which high public revenues volatility can reduce the efficiency of public policy and, as a consequence, directly lower economic growth, without going through the channel of macroeconomic volatility discussed above. The mechanism is the following: volatility in public income lowers re-election probability by reducing the benefit of staying in power; this, in turn, reduces the (political) cost of inefficient policies, such as patronage and clientelism, thus negatively affecting economic growth.

Third, the tax-smoothing literature pioneered by Barro (1979) and Lucas and Stokey (1983) shows that deviations from the optimal path of keeping tax rates (the share of income paid into taxes) stable over the business cycle will lead to additional distortions and welfare losses for any given level of government spending. Thus, even dismissing a Keynesian approach, optimal fiscal policy requires the ability to borrow during slumps to allow taxes to fall in line with output while keeping public expenditures stable.[41]

Finally, in countries which raise small amounts of revenues compared to the size of the economy – as was the case almost everywhere in the interwar period, and is the case in many developing countries today – sudden falls in government financing can disrupt the very functioning of states and their ability to provide public goods. In extreme cases, this might lead to regime changes. Although no causal claim is made in this paper, I note in passing that in the early 1930s many countries around the world experienced dramatic regime changes. The case of Germany with the rise to power of the NSDAP in 1933 is notorious, but further regime changes and general increases in autocracy took place in many countries in Eastern and Central Europe and Latin America, for example. de Bromhead, Eichengreen, and O'Rourke (2013) analyze the link between economic hard times and right-wing extremisms during the interwar period and find that where depressed economic conditions were allowed to persist, the wise of extremism was more likely. The

[41] This result can change if expenditure is endogeneized, see Ferriére and Karantounias (Forthcoming).

inability to prevent a collapse in government financing may have played a role in this.[42]

An issue in this research, as in much of the macroeconomics literature, is that taxation, and the revenue side of fiscal policy more generally, are only discussed in light of potential distortionary effects. The origin of countries' ability to raise revenue and spend are not normally considered. This means that some key questions remain without answer. Under which conditions will the public sector be able to smooth its revenues effectively and be more stable than the private one? Does the ability to sustain automatic stabilizers or run an effective fiscal policy depend on the size of the government itself? What are the determinants of the fiscal development of states? This paper offers some insight into these questions against the backdrop of the largest and most widespread peace-time economic downturn in history.

4.3 New data and some descriptive statistics

Existing datasets on fiscal variables in the interwar period are incomplete. The most comprehensive sources collecting international data are Mitchell's International Historical Statistics volumes (Mitchell, 2007) and Flora et al's data handbook on Western Europe (Flora, Kraus, and Pfenning, 1987). However, these do not provide detailed information on tax structures and are almost exclusively focused on central governments.

I have transcribed data from various publications of the German Imperial Statistical Office (Statistiches Reichsamt) on the size and composition of countries' central government tax revenues, as well as some more limited information on local governments. The local level data remains incomplete and the econometric analysis below focuses on central governments. Nonetheless, I draw some interesting stylized facts from both central and local data, which improve our understanding of taxation in the interwar era. Directly below, I discuss my two fiscal capacity indicators: tax revenues as a share of GDP and income tax revenues as a share of GDP. I also provide a snapshot of the size of local governments in the interwar period. In Appendix 4.B, I discuss the other data and the sources in detail.

Table 4.1 offers a comparative picture of tax revenues as a share of GDP. Direct comparisons across countries are imperfect due to different accounting standards, but are nonetheless useful. The very low incidence of taxation by the standard of today's developed countries is immediately evident. Rich countries today tax around 40% of

[42] Other past regime changes have also been linked to fiscal factors, for example the French Revolution (Tilly, 1975; Sargent and Velde, 1995).

Table 4.1: Tax revenues as a percentage of GDP

	Central tax revenue/GDP				Central & Local tax revenue/GDP			
	Pre-GD	1929-33	Post-GD	Average	Pre-GD	1929-33	Post-GD	Average
Austria	8	10	15	10		15	21	16
Belgium	5	7	10	7	5	8	11	8
Bulgaria	7	6	8	7			8	8
Czechoslovakia	10		17	14				
Denmark	7	5	6	6	12	9	10	10
Finland	7	6	7	7				
France	9	13	16	12		15	22	17
Germany	3	7	12	7	12	15	22	17
United Kingdom	14	14	16	14		17	19	18
Greece		8	5	7				
Hungary	10	9	15	11		12	20	15
Ireland		14	20	17				
Italy	7	9	14	10	10	14		12
Netherlands	6	6	9	7	8	10	16	10
Norway	10	8	8	8	20	14	14	16
Poland	5	7	10	8		13	13	13
Romania	14	15	15	15				
Spain	9	8		9				
Sweden	6	6	7	6	10	10		10
Switzerland	3	4	6	4	7	10		9
Yugoslavia		11		11				
Canada	6	6	7	6				
USA	3	3	5	4	8	12	12	10
Argentina	5	4	6	5				
Brazil		4	5	4				
Chile	8	8	7	8				
Colombia	5	5	3	4				
Australia	6	7	6	6				
India	2	2	2	2				
Japan	6	5	4	5				
Turkey		6	13	8				
Egypt	10	10	14	11				
South Africa	7	7	10	8				
Average	7	7	10	8	10	11	13	12
Western Europe average	7	7	10	8	9	11	15	11
European core average	7	9	13	9	9	13	18	14
Eastern Europe average	9	9	13	11		11	17	12
Latin America average	6	5	5	5				

Source: author's estimates, see Appendix 4.B for detail on the sources. The post-Depression figure for the United States is from the Historical Statistics of the United States, millennial edition (Wallis, 2006). Pre-GD=1918-28. Post-GD= 1934-38. Western Europe: Austria, Belgium, Denmark, Finland, France, Germany, UK, Italy, Netherlands, Norway, Spain, Sweden, Switzerland. European core: Belgium, France, Germany, UK, Italy, Netherlands. Eastern Europe: Bulgaria, Czechslovakia, Hungary, Poland, Romania. Latin America: Argentina, Brazil, Chie, Colombia.

GDP on average (Besley and Persson, 2014). The average incidence of central government taxation between 1918 and 1928 was around 7% and had increased to 10% in 1934-1938. These levels are similar to those of low-tax developing countries today. The inclusion

of local level taxation for the countries for which these data are available only increases the fiscal capacity indicator to 15% and 12% in post-Depression Western Europe and US respectively.

However, a clearly divergent trend emerges when Europe and the United States are compared to the rest of the world. In Western Europe, the incidence of taxation at the central level increased on average from 7% before the Great Depression to 10% after, and from 10% to 13% when local governments are included. The incidence of taxes rose in almost all the Western European countries considered in the sample, the exceptions being Denmark, Finland, Norway, and Greece. In the US the increase was from 3 to 5% and 8 to 12% respecively. Data on government financing are more complete than the tax data and allows a broader comparisons of public finances at the local level. When the tax and financing data overlap, they offer a very similar picture. However, the inclusion of local level financing means that Canada can now clearly be assimilated to the European and US experience of relatively high and increasing revenues levels. These increases were particularly strong in the post-Depression era. No similar general upward trend is visible in the rest of the world, with the exception of Egypt and South Africa.

The relatively high and increasing taxation levels of Eastern Europe are also noteworthy. These were slightly greater than those of Western Europe as a whole, but lower than those of the European core constituted of Belgium, France, Germany, Italy, Netherlands and the United Kingdom. The two African countries in the sample, Egypt and South Africa, also stand out for their generally high taxation levels, given their level of economic development. This could be at least partially explained by the fact that they were a British protectorate and a recently independent British ex-colony respectively. In Asia, Japan's taxation levels are below average, while India's are the lowest of the whole sample. Unfortunately, the coverage of Asia and Africa is limited to these four countries and to the central government level due to data availability. This rules the assessment of broader developments in the two continents.

The most striking aspect of the table, however, is Latin America's overall very low and stagnant level of taxation, which has persisted, in relative terms, until today (Sokoloff and Zolt, 2007). The only partial exception is Argentina, a country which introduced substantial reforms – for example by implementing its first income tax – as a result of the Depression.

The increase in fiscal capacity in Europe and North America documented in the table suggests that the staggering increase in the incidence of taxation, which took place over

the course of the 20th century, was already well under way in the interwar era. Research on the US has fidentified the Great Depression as a crucial episode in triggering an increase in the role of government in the economy (Wallis, 2000; Gillitzer, 2017), and it would appear that a similar pattern can also be identified for Europe.

Table 4.2: Income (direct) taxes as a percentage of GDP and total tax revenues

	Income Tax Revenue/GDP				Income Tax Revenue/Total Tax Revenue			
Country	Pre-GD	1929-33	Post-GD	Average	Pre-GD	1929-33	Post-GD	Average
Austria	2.5	2.4	2.9	2.5	30.4	24	19.2	26.1
Belgium	1.9	2.4	3	2.3	37.9	33.3	30.3	33.3
Bulgaria	1.4	1.6	1.8	1.7	21.5	26.3	20.5	23
Czechoslovakia	2.1		3.1	2.6	20.4		17.7	19.1
Denmark	2.6	1.7	1.8	1.9	38.4	34.1	30.2	33.8
Finland	1.6	1.4	1.4	1.5	22.1	23.8	20.5	22.1
France	3.3	4	4.7	3.9	35.6	32.2	29.1	32.6
Germany	1.1	1.5	6	2	19.1	22	50	24.8
United Kingdom	7.9	7.6	7.8	7.7	57.3	56.3	49.6	53.5
Greece		1.6	1.2	1.4		19.7	22.8	21
Hungary	2.4	3	4.8	3.4	23.9	33.2	31.7	30.6
Ireland		3.9	4.9	4.4		27.9	25.1	26.5
Italy	2.8	2.8	3.5	3.3	39.4	32	25.4	32.8
Netherlands	2.5	2.5	2.6	2.5	45.3	40.5	27.5	38.4
Norway	4	2.4	2.6	2.7	38.4	31.7	30.3	31.7
Poland	1.7	2.3	3.4	2.5	31.6	30.8	34.4	32.2
Romania	3.8	4.2	3.1	3.5	26.9	29.2	20.9	24.1
Spain	2.9	2.8		2.8	32.4	33.2		32.8
Sweden	1.8	1.5	1.8	1.7	28.3	26	23.9	25.5
Switzerland	0.5	0.5	0.7	0.7	17.2	13.1	12.6	13.6
Yugoslavia		2.5		2.5		22.9		22.9
Canada	1	1.3	1.6	1.2	17.4	20.9	22.9	19.9
United States of America	2.2	2	2.4	2.2	66.1	60.9	47.9	58.7
Argentina	0.2	0.4	1	0.5	4.3	8.8	17.1	10
Brazil		0.2	0.4	0.3		6.8	9.1	8
Chile	1.8	2.4	1.8	2	21.5	30.2	24.4	26.1
Colombia	0.2	0.4	0.5	0.4	4.8	9.8	16	10.3
Australia	1.6	2	1	1.4	26.3	28.8	17.8	22.5
India	0.3	0.4	0.4	0.4	21.6	22.1	21.6	21.7
Japan	2.1	1.7	1.3	1.7	32.9	30	39.1	32.6
Turkey		1	4.5	2.4		17.1	35.8	23.3
Egypt	2.8	2.8	3.9	3.2	29.3	27.8	27.3	28.1
South Africa	3	3.9	5.9	4	42.8	43.1	56.1	47.9
Average	2.2	2.2	2.8	2.3	29.8	28.1	27.6	27.6
Western Europe average	2.5	2.4	3	2.5	31.6	28.7	26.8	28.7
European core average	3.3	3.5	4.6	3.6	39.1	36.1	35.3	35.9
Eastern Europe average	2.3	2.8	3.2	2.7	24.9	29.9	25	25.8
Latin America average	0.7	0.9	0.9	0.8	10.2	13.9	16.6	13.6

Source: author's estimates, see Appendix 4.B for detail on the sources. The post-Depression figure for the United States is from the Historical Statistics of the United States, millennial edition (Wallis, 2006). Pre-GD=1918-28. Post-GD= 1934-38. Western Europe: Austria, Belgium, Denmark, Finland, France, Germany, UK, Italy, Netherlands, Norway, Spain, Sweden, Switzerland. European core: Belgium, France, Germany, UK, Italy, Netherlands. Eastern Europe: Bulgaria, Czechslovakia, Hungary, Poland, Romania. Latin America: Argentina, Brazil, Chie, Colombia.

Tables 4.2 offers an even more more clear-cut picture of the difference in taxation patterns across countries by presenting information on income (direct) taxes. Europe, and the European core in particular, stand out for their high level of direct taxation both as a share of GDP and as a share of overall tax revenue. As expected, the United Kingdom emerges as the country with the most developed system of income taxation. Latin America, instead, is confirmed to be lagging behind severely in the development of its tax system, with the partial exception of Argentina.

The dynamics of direct taxes offer an interesting picture as well. In most European countries – the exceptions are Germany, Greece, Hungary and Poland – and in some non-European countries – the US and Australia – the share of income taxes in overall tax revenues decreased significantly over the course of the interwar period, even if their share in GDP tended to increase. This partly reflects a return to lower levels of income taxation after the fiscal strain of WWI, which relied heavily on increases in direct taxes. The expansion of indirect taxes – such as taxes on sales, consumption, capital transactions and transportation – also played an important role in the decrease in the relative importance of income taxation in this period. The picture for custom duties, instead, is less clear cut. In general, they lost some relevance in this period, but the pattern is different in every country. This illustrates the complex evolution of tax systems over the course of the interwar period.

Table 4.3 offers a snapshot of the size of local governments in the early 1930s by illustrating the local share of government financing, taxes and public debt over their respective nationwide totals. The table demonstrates that the size of local governments varied widely across nations, with relatively centralized countries – such as Belgium, Bulgaria, Italy and France – on one end of the spectrum and decentralized ones – such as Australia, Brazil, the Netherlands and Germany – on the other. Noteworthy is also the high degree of decentralization of the relatively sparsely populated Scandinavian countries. The three indicators in the table broadly tell the same story: countries with relatively larger local revenues had higher shares of local taxation and their local governments also had larger debts, but the relationship is not one-to-one. Many factors played a role in determining the degree of fiscal federalism in each country. As I argue in Section 4.4.4, one of these is the incidence of natural disasters, which I exploit in my instrumental variable analysis.

Table 4.3: Local government financing, taxes and debt as a percentage of their respective totals

Country	Year	GovtFin	Taxes	Debt	Country	Year	GovtFin	Taxes	Debt
Argentina	1931			20	Ireland	1931	31		37
Australia	1931	68		48	Italy	1932	33	21	9
Austria	1931	37	35	20	Japan	1931	56		26
Belgium	1931	21	12	1	Latvia	1931	37		
Brazil	1931	40		77	Lithuania	1931	8		
Bulgaria	1932	18	14	5	Netherlands	1931	64	37	46
Canada	1931	56	58	48	New Zealand	1931			19
Chile	1931	21			Peru	1931			4
Colombia	1931	41		42	Poland	1933	24	20	18
Czechoslovakia	1930-1933	25		23	Spain	1931	18		
Denmark	1931	69	44	44	Sweden	1931	51	39	46
Estonia	1931	21			Switzerland	1931	70	61	35
Finland	1931	21		47	Turkey	1931	21		
France	1930	13	12	3	South Africa	1931	46		
Germany	1931	60	53	54	Uruguay	1931			18
Greece	1931	6			United Kingdom	1931	40	19	16
Hungary	1931		26	32	United States	1932	41	70	50
India	1931	52			Yugoslavia	1931	20		

Source: Author's estimates. For the sources of the debt and financing/tax data see Appendix 3.B and 4.B respectively. The tax figure for Canada is from Sokoloff and Zolt (2007). The post-Depression figure for he United States is from the Historical Statistics of the United States, millennial edition (Wallis, 2006).

4.4 Empirical strategy

This section outlines the empirical strategy of the principal analysis of this paper. Its objective is detecting and quantifying the effect of pre-existing tax structures, in conjunction with changes in economic activity, on changes in tax revenues and government financing.

More precisely, I study the impact of fiscal capacity on tax revenues and on a composite made up of tax revenues, non-tax government income – i.e. the profits of publicly owned corporations and public monopolies and other unclassified revenues including the sales of some commodities abroad – and long-term – over 1 year maturity – borrowing. I refer to this composite as simply government financing. As mentioned above, the latter variable represented the bulk of resources at the disposal of governments in the interwar period – 96% on average between 1927 and 1938 – while tax revenues represented on average 73%. Governments also relied on short-term borrowing from various sources – including central banks – to ramp-up spending within short time horizons.

The results indicate a major role for fiscal capacity in reducing the instability government financing, while the results for just tax revenues are not conclusive. This suggests

that high capacity countries were able to smooth their public revenues though non-tax revenues, as would be desirable in an optimal fiscal policy scenario. I ascribe these findings to the impact of fiscal capacity on countries' ability to borrow and explore this channel in detail in Section 4.6. I conclude that fiscal capacity, by signaling higher institutional quality, allowed countries to borrow more extensively and more cheaply. These findings strongly suggest that policy responses during the interwar period were constrained by preexisting fiscal systems, and not only Gold Standard membership and ideology as suggested by much of the literature. Faced with dwindling tax revenues and the inability to tap financial markets, some countries were not in the position of running expansionary fiscal policies – or even simply avoid a collapse in public expenditure – even if they had wanted to and regardless of their commitment to gold.

The section is structured as follows. I firstly discuss the model used in the estimations (Section 4.4.1). I then discuss two important issues: the interpretation of the fiscal capacity coefficients (Section 4.4.2), and the possibility of reverse causality between my outcome variables and output (Section 4.4.3). Finally, I outline my instrumental variable (IV) strategy (Section 4.4.4).

4.4.1 Model

The basic empirical approach consists in relating annual changes in public revenues to annual changes in economic activity measured by GDP. This approach is common in the empirical literature on the elasticity of revenues to changes in output. For an early example see Vogel and Trost (1979)'s study of the elasticity of tax receipts to changes in income in US states between 1957 and 1975; Kodrzycki (2014) applies the methodology to contemporary US states revenues, Ilzetzki and Vegh (2008) use it in a cross-country setting and Brückner (2012) employs to focus on sub-Saharan African countries. In a different, but connected application, Vegh and Vuletin (2015) use it to study the cyclicality of tax rates, rather than revenues, across countries. Following the literature, I study revenues as a whole rather than estimating an equation for each of their components separately. The former is preferable due to the interdependence between different types of receipts, which would make ex-post aggregation problematic using the latter approach.

I introduce several innovations compared to previous studies. Firstly, to my knowledge, I am the first to investigate the impact of fiscal capacity on government revenues volatility. Secondly, I control for the composition of tax revenues, the degree of fiscal capacity and the other control variables by keeping them constant at their average values

before the period of observation. I adopt this strategy to reduce the risk of reverse causality – countries might choose a particular tax system because of changes in their revenues – and to deal with confounding effects due to idiosyncratic changes in the regressors unrelated to true changes in fiscal institutions. This is important because my objective is establishing how the persistence of fiscal systems locks-in countries leading to differences in the response of revenues to economic shocks. Thirdly, I deal with potential endogeneity by instrumenting fiscal capacity with the time countries spent fighting major external conflicts, in line with the literature studying the impact of fiscal capacity on long-term development (Dincecco and Prado, 2012), and by introducing a novel instrument: the incidence of natural disasters.

I begin the analysis by simply estimating the elasticity of tax revenues and government financing to economic activity. Economic activity is measured by the nominal, non-PPP-adjusted GDP provided by Klasing and Milionis (2014) (NGDP). This is a useful indicator given that changes in revenues are driven by both changes in real economic activity and prices. The alternative would be to use changes in real GDP and convert the revenues into constant prices, but the choice of deflator is problematic and particularly challenging in the strong deflationary context of the the Great Depression. Even with an adequate deflator, the PPP adjustment would have to be accounted for. Using Klasing and Milionis' data represents the most straightforward solution. The differences models is outlined in equation 4.1 where c_i are country fixed effects, l_t are year fixed effects and $\epsilon_{i,t}$ are idiosyncratic disturbances.

$$\Delta Revenue_{i,t} = \beta_0 + \beta_1 \Delta NGDP_{i,t} + c_i + l_t + \epsilon_{i,t} \tag{4.1}$$

I retain the differences specification for the whole analysis in order to account for the probable persistence and non-stationarity of the data. Due to the large size of the swings in the dependent variables, I use percentage changes rather than log differences given that the latter represent precise approximations only for small percentage changes.[43]

The inclusion of country fixed effects allows to control for a vast array of time invariant or slow moving country characteristics – including geography, demography and economic structure – and to thus minimize the danger of omitting a variable correlated with the regressors and causal for the dependent variable. Time fixed effects allow me to account for common shocks. Panel estimation also minimizes issues of cross-country comparability of

[43]Both are straightforward solutions to problems often ignored in the literature. I cluster the standard errors at country level to account for heteroskedasticity.

the data by exploiting the time series rather than cross-sectional variation of the variables. Although the within (FE) and first differences (FD) estimators both account for country fixed effects, the underlying assumptions for unbiasedness and consistency are weaker for FD: whereas the regressors have to be uncorrelated with idiosyncratic errors in the past, present and future for FE, for FD these just need to be uncorrelated with the current, previous and next disturbances. The FD estimator thus provides a more reliable estimator of causal effects, but this comes at the cost of less efficiency, since first-differencing substantially reduces the variability of the regressors, leading to larger standard errors. Throughout the analysis, I will present both FE and FD estimations, using them in conjunction to provide a thorough interpretation of the results.

After estimating simple elasticities, I expand the model include my fiscal capacity indicator. In addition, I control for the structure of tax revenues – since these are potentially correlated with both the outcome and the capacity indicator – and a series of other controls included in vector $\mathbf{x}$. The structure of tax revenues is measured as a the share of income and the share of indirect taxes in total tax revenue. Custom duties are excluded to avoid collinearity, but the results are robust to their inclusion in place of either the income or indirect tax variables. The rest of the right hand variables are discussed throughout the analysis, and described in detail in Appendix 4.B.

As mentioned above, I use two indicators of fiscal capacity: taxes as a share of GDP and income taxes as a share of GDP. The objectives of the paper is to estimate the effect of fiscal capacity separately from the effect of the composition of tax revenues. The two effects are different because the former regards the reaction of revenues to changes in a particular tax base (income) while the latter deals with the more general way in which the level of development of a tax system influences changes in revenues. The empirical analysis demonstrates that, indeed, the income tax share of revenues measures something different from the fiscal capacity indicators, at least in the interwar context. I find no significant connection between the share of income taxes in tax revenue and government financing volatility, however measured. It appears that for the purpose of determining the effect of fiscal capacity on the cyclicality of revenues, the share of income taxes in revenue is not an appropriate indicator of fiscal capacity.

The model takes the following form.

$$\Delta Revenue_{i,t} = \beta_0 + \beta_1 \Delta NGDP_{i,t} + \phi_1 FiscalCapacity_{i,av} + \phi_1 IncomeTaxShare_{i,av} + \phi_2 IndirectTaxShare_{i,av} + \mathbf{x_{i,av}}\gamma + c_i + l_t + \epsilon_{i,t} \tag{4.2}$$

The av subscripts on the regressors indicates that they are 1914-1926 averages, while the regressions are run from 1927 to 1938. Unfortunately, observations before 1926 are limited, so the averages at times refer to 1 to 3 observations only. For robustness, I also run the model for a longer sub-period, 1921-38, using alternatively full sample averages (1914-38) and 5-year moving averages as regressors. The results are very similar.

The model of equation 4.2 is not yet satisfactory for three reasons. First, it simply estimates whether, given a certain fiscal structure and a certain change in economic activity, changes in revenues are smaller or larger on average. This is not particularly informative given that changes in activity and revenues can be both positive and negative. The result of the estimation will be a simple average of the two. Second, there is no reason to believe that the response of revenues to changes in economic activity should be linear. One could expect large shocks to be different from small shocks. Third, the presence of time-invarying regressors makes the estimation problematic because, with standard panel data methods, the ϕ coefficients cannot be estimated. Failing to quantify these might lead to a partial picture of the marginal effect of fiscal systems on revenues. My solution to overcome all these issues is twofold. First, similarly to Brückner (2012), I interact the percentage change in economic activity (ΔNGDP) with all the other regressors. The result is that all the variables become time-varying and that I can now identify non-linear relationships in the data. Second, I employ the the Hausman and Taylor (1981) (HT) approach alongside standard estimators. This allows me the obtain the coefficients of time-invarying regressors, while controlling for fixed-effects.

The model is now as follows:

$$\Delta Revenue_{i,t} = \beta_0 + \beta_1 \Delta NGDP_{i,t} + \phi_1 FiscalCapacity_{i,av} + \phi_2 IncomeTaxShare_{i,av} + \phi_3 IndirectTaxShare_{i,av} + \mathbf{x_{i,av}}\gamma + \Delta NGDP_{i,t} * \mathbf{z_{i,av}}\theta + c_i + l_t + \epsilon_{i,t} \tag{4.3}$$

where $\mathbf{z} = \{FiscalCapacity, IncomeTaxShare, IndirectTaxShare, \mathbf{x}\}$.

In this model, the marginal effect of the variables changes with the value of the variable with which the interaction occurs. For example, the marginal effect of $\Delta NGDP$ is now $E(\Delta Revenue|\Delta NGDP+1,\mathbf{z})-(\Delta Revenue|\Delta NGDP,\mathbf{z})=\beta_1+\mathbf{z}\theta$, where the vector $\mathbf{z}$ is held constant at a specific value. In turn, the marginal effect of the element j of $\mathbf{z}$ is $E(\Delta Revenue|z_j+1,\Delta NGDP)-E(\Delta Revenue|z_j,\Delta NGDP)=\phi_j+\theta_j\Delta NGDP$, where this time $\Delta NGDP$ is held constant. More details on interpretation and some numerical examples are provided in Section 4.5.4.

4.4.2 Interpreting the fiscal capacity coefficients

The main coefficients of interest in the analysis illustrate the reduced form relationship between fiscal capacity (interacted with changes in NGDP or not) and changes in tax revenues and government financing. The underlying hypothesis is that fiscal capacity will affect the extent to which revenues change following shocks to economic activity. In the interwar context, it is sensible to interpret yearly changes in public revenues as a combination of three factors: 1) automatic reactions to changes in the tax base (i.e. the elasticity), 2) changes in fiscal policy; 3) a general positive trend in public revenues (to the extent that this does not get filtered out by first differencing). The first two elements depend directly on changes in economic activity, so that:

$$\Delta Revenue=\Delta NGDP\times\{Elasticity+PolicyReaction\}+Trend$$

A distinction can be drawn between the two outcome variable of the analysis. In the case of tax revenues, for policy reactions to play an important role one would need to assume that fiscal policy in the interwar period responded in a systematic way and with no lag to economic conditions. Based on what we know, this is not a realistic assumption. Fiscal policy was essentially not employed as a tool if not in an extremely limited way and in a handful of countries. Delay in budgetary processes and parliamentary approval would have made it very difficult for governments to change tax policy to respond contemporaneously to changes in economic conditions. Moreover, although the monitoring of economic activity made significant progress in some countries in the interwar era, this was still a relatively new and limited phenomenon making it difficult for governments to respond in real time. In particular, the concept of GDP did not exist and it is thus difficult to fathom the existence of fiscal policies designed to respond to yearly changes in this variable.

In summary, it is implausible to assume systematic and contemporaneous policy reac-

tions in terms of tax law changes to changes in economic activity in the interwar period. Therefore, the bulk of the variation in tax revenues should reflect the elasticity of tax revenues to changes in economic activity and the trend. In this scenario, fiscal capacity should affect tax revenues only to the extent that it captures the breadth of the tax base. Since I control for the structure of tax revenues, however, the expectation is that fiscal capacity should not affect the response of tax revenues to changes in output. This is indeed the case, as my IV results demonstrate.

When considering government financing, however, the role of policy reactions can no longer be ignored. This is because the amount a country borrows (a component of the financing variable) is a policy choice, which needs to be taken however imperfect the information available to the policy maker might be. This choice can be further broken down into two components. One is the extent to which a country smoothes its revenues on average through borrowing in response to changes in economic activity, which depends on country specific factors, such as fiscal capacity. The other is discretionary changes in the preference for borrowing. The smoothing effect of fiscal capacity on government financing represents my quantity of interest, whereas the discretionary component, if unrelated to fiscal capacity or the other regressors, will simply end up in the error term without affecting the estimates. With the inclusion of an adequate set of controls, which account for country characteristics and changing economic and political conditions, the model outlined above should be able to capture precisely the magnitude of interest. Indeed, the smoothing effect of fiscal capacity on financing emerges strongly and clearly throughout the whole analysis.

4.4.3 Reverse causality between changes in revenue and economic activity

Although estimating the relationship between changes in economic activity and revenue is the not the primary goal of this paper, it is nonetheless important to insure that reverse causality between economic activity and revenues does not mar the estimation. This is because the resulting bias might affect the other coefficients as well. Fortunately, there are substantial reasons to exclude dangers to inference.

First, for the reverse causality between economic activity and revenues to be a concern, one would have to assume that NGDP in the interwar period responded in a systematic way and with no lag to changes in public revenues. Leaving aside the long-standing and so far inconclusive empirical and theoretical debates on the effects of fiscal policy on economic activity, there is no strong theoretical prior regarding the impact of an increase

in revenue, narrowly or broadly defined, on GDP without regard to what is happening on the expenditure side. Changes in revenue alone tell us nothing about the stance of policy. The correlation between changes in my government financing variable and expenditure, albeit strong (0.624), is far below 1. As mentioned earlier, changes in expenditures were influenced by short term borrowing, which is excluded from my analysis. The correlation between changes in tax revenue and expenditure, instead, is low at 0.158.

Second, our knowledge of the Depression years indicates that changes in output were most likely little related to fiscal policy. As discussed above, in most cases fiscal policy was not used. In the few cases in which countries did run deficits, these were too small to be effective. (Almunia, Bénétrix, Eichengreen, O'Rourke, Rua, Tenreyro, and Perri, 2010).

Third, while basic economic theory tells us that a higher tax burden will distort economic activity leading to lower output, the regression coefficients do not square with causality running from changes in revenue to changes in output in the interwar period. Whereas one would expect a negative correlation between changes in tax revenues and GDP, this is positive, and strongly so. Moreover, one would expect changes in government financing (which includes long term borrowing) to be more positively correlated with GDP than tax revenues, since more borrowing should lead to more public expenditure and higher output. However, I find the opposite: government financing is less strongly positively correlated with GDP than tax revenues. This indicates that causality ran from changes in economic activity to changes in tax revenues, which were then partially mitigated by non-tax revenues.

Despite this evidence, worries that changes in revenue could affect economic activity remain. As a final reassurance that my results are not affected by reverse causality, I run a robustness check in which I use a similar methodology to Ilzetzki and Vegh (2008): I instrument changes in output with average changes in the nominal GDP of trading partners weighed by the pre-Depression share of exports going to that country. By using this methodology, I can also employ expenditures directly as my outcome variable without worries about reverse causality. I report the results of this exercise, which yields very similar results to the baseline analysis, in Appendix 4.A.

4.4.4 IV strategy

The main empirical relationship of interest, that between changes in revenues and fiscal capacity, might also be affected by endogeneity. A plausible way in which this can operate

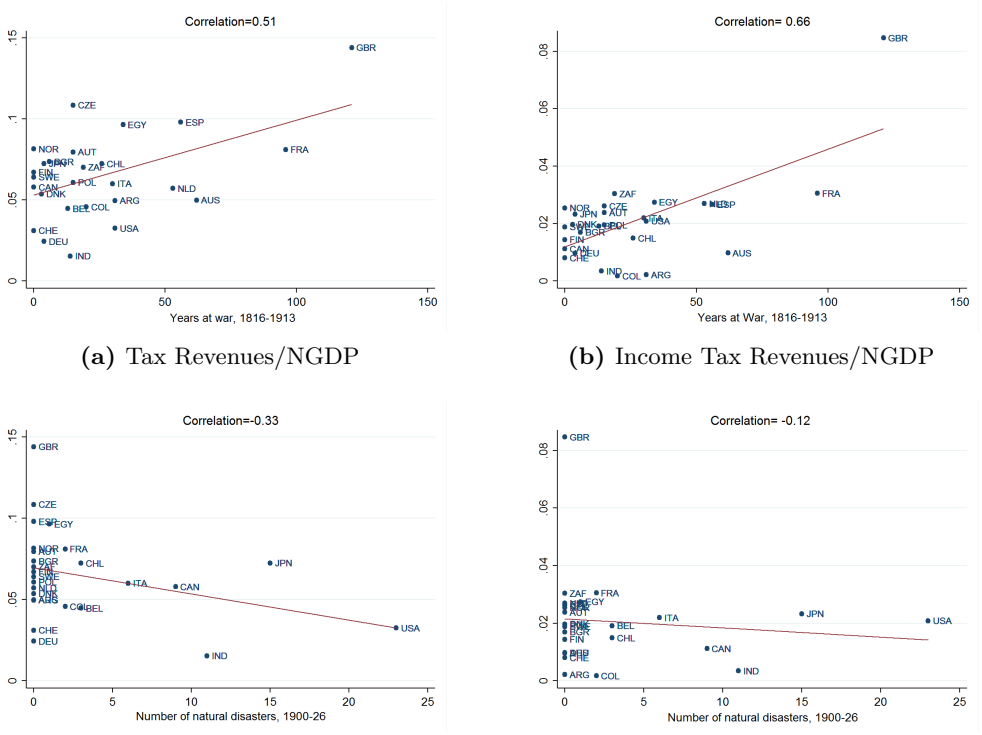
is that countries with a higher revenue volatility might invest more in fiscal capacity in order to reduce it. In this case, the effect of fiscal capacity on changes in revenue would be underestimated and biased towards zero. The issue of the potential co-determinateness of economic volatility and government size arises also in other contexts, as demonstrated by Rodrik (1998)'s study on the effect of openness on government size. Another potential source of endogeneity, of the opposite sign, is that a higher revenue volatility might make investments in fiscal capacity more difficult. Thus, the direction of potential bias is a priori unclear. In order to overcome this threat to causal inference, I employ an instrumental variable approach.

I use two variables to instrument fiscal capacity. The first is the number of years a country spent fighting major external conflicts between 1816 and 1913, as reconstructed by Dincecco and Prado (2012). The use of this variable follows a large literature linking armed conflicts, fiscal reforms and fiscal development. The second is a novel instrument which, I argue, captures a different mechanism of fiscal capacity formation. The variable is the number of natural disasters to hit a country between 1900 and 1926.

Figure 4.2 illustrates the unconditional correlation between the instruments and my two fiscal capacity indicators. This is positive and strong for the conflict variable (panels 4.2a and 4.2b) even if the United Kingdom, an outlier with significant leverage, is excluded. The correlation is negative and somewhat weaker for the disasters variable (panels 4.2c and 4.2d), but the analysis shows that both variables represent relevant instruments for fiscal capacity when used alone and in conjunction. Directly below, I outline why the instruments are relevant and valid, starting with the conflicts instrument and proceeding with the disasters variable.

The warfare instrument

In their work, Dincecco and Prado (2012) use the incidence of major past external conflicts, which are more likely to have led to substantial and lasting fiscal reforms compared to small conflicts, as an instrument for contemporary fiscal capacity in order to quantify its effect on long-term development. The authors construct their instruments from Clodfelter (2002)'s database of major external conflicts in Europe, Africa, the Middle East, Asia and Oceania between 1500 and 2000. Their preferred instrument is the number of casualties due to major external conflicts between 1816 and 1913 normalized by country size, while their fiscal capacity indicator of choice is the share of direct taxes in total tax revenue. My instrument of choice, instead, is one they construct for robustness: the number of years a



(a) Tax Revenues/NGDP **(b)** Income Tax Revenues/NGDP

(c) Tax Revenues/NGDP **(d)** Income Tax Revenues/NGDP

Figure 4.2: Fiscal capacity, the incidence of external conflict and natural disasters

The years at war variable can exceed 97 if countries were involved in more than one conflict in a given year. Source: The years of war data is from Dincecco and Prado (2012), the natural disasters data is from the EM-DAT dataset of the Centre for the Epidemiology of Disasters (CRED, 2017), the GDP data is from Klasing and Milionis (2014), for the historical tax revenue data see the text and Appendix 4.B for details.

countries spent at war between 1816 and 1913. I choose this variable over the casualties indicator due to its strong correlation with my fiscal capacity indicators. The casualties variables, instead, is very weakly related to these.

For the warfare instrument to be relevant, different degrees of engagement in external conflicts between 1816 and 1913 need to have led to the accumulation of different degrees of fiscal capacity, which then persisted at least until the interwar period. For the instrument to be valid three conditions need to be met, conditional on controls: 1) the incidence of conflicts between 1816 and 1913 must have had no effect on changes in government revenues between 1927 and 1938 except through its effect on interwar fiscal capacity; 2) changes in revenues in 1927-38 are not related to the incidence of conflicts in 1816-1913; 3) there is no reverse causality between fiscal capacity and warfare. A violation of the first condition could occur if, for example, a higher incidence of external conflicts raised the probability of a country gaining (losing) access to a revenue source that was more (less) volatile than its existing tax base. This could be a territory, a port, or a tradable natural resource. The second condition could be violated if the volatility of revenues in

1927-36 was correlated through persistence to the volatility of revenues in 1816-1913 and this in turn influenced the probability of fighting wars in this period. Finally, and more worryingly, the exclusion restriction would be violated if fiscal capacity drove the decision to engage in conflicts rather than vice versa.

Based on a large body of research by historians, economic historians and other scholars, Dincecco and Prado argue that "war participation drove fiscal capacity improvements, but that capacity constraints themselves did not significantly influence whether rulers went to war." (page 175). Indeed the work of authors such as Brewer (1990), Tilly (1975, 1990), Hoffman and Rosenthal (1997), O'Brien (2011), Dincecco, Federico, and Vidigni (2011) and Hoffman (2015) indicates that in the early modern and modern period the incidence (and threat) of war stimulated fiscal reforms and other innovations to increase fiscal capacity, particularly in Europe.[44] Recent empirical evidence seems to support the existence of a link between warfare and the development of fiscal capacity although this might not always persist (Sabaté, 2016), lead to development of general interest states (Dincecco, Fenske, and Onorato, 2016), or be as strong outside of Europe (see Centeno (1997, 2002); Gupta, Ma, and Roy (2016) and references in Dincecco (2015), page 909-11).

From a theoretical perspective, Besley and Persson (2010) conceptualize this mechanism by arguing that war leads to an exogenous increase in the demand for a public good – defense – which needs to be financed through taxation. The increase in fiscal capacity due to war, is then maintained and amplified due to the dynamic interactions between fiscal capacity, legal capacity and development.[45] Alesina, Reich, and Riboni (2017) offer a complementary perspective by arguing that warfare can shift public expenditure towards public goods during times of war in order to foster support in the population.[46]

Naturally, countries that faced less political resistance to increases in taxation would have been able to raise more tax revenues for any given incidence of external conflicts

[44] Dincecco and Prado make an important distinction between external and internal conflicts. While the latter contribute to the strengthening of fiscal and state capacity, the former can severely damage it. See also Besley and Persson (2010) on this point.

[45] Dincecco and Prado illustrate two further channels through which increases in fiscal capacity might persist after the end of conflicts. First, after the creation of strong and centralized fiscal institutions due to wars "the new executives inherit strong fiscal institutions, they may wish to exploit them for their own purposes rather than cede authority back to traditional elites." Second, the changing nature of warfare in the 19th century may have led to the elites conceding an increase in the resources made available for taxation in a bargain with the rulers. Once these changes were implemented, the new taxes might have been redirected towards public services valued by the elites, rather than the taxation being rolled back to pre-war levels.

[46] More precisely, the authors argue that states will attempt to modify the degree of political resistance in the population by employing positive and/or negative nationalisms. The former is tied to creating a national identity and a common language through the provision of public goods. The latter consists of aggressive negative propaganda towards to opponent and is the go-to option when the capacity to provide mass public goods is absent.

(O'Brien, 2011; Hoffman, 2015). Moreover, Queralt (2017) argues that the ability to rely on borrowing to finance wars might undermine the impact of warfare on fiscal capacity development since countries can default after the end of conflicts instead of increasing tax revenues. These considerations are not inconsistent with the causality running from conflict to capacity, it simply means that the effects of warfare were not homogeneous across countries due to other factors also playing a role. Gennaioli and Voth (2015), however, qualify these arguments in a theoretical model that incorporates the idea that causality might also run from fiscal and state capacity to conflict. Their key insight is that the threat of war after the Military Revolution, which made monetary resources more important in conflicts, led to cohesive states investing in state building in order to fight wars, while divided ones dropped out of the competition. Thus, they argue, conflict might not necessarily lead to the accumulation of fiscal capacity. It is the underlying characteristics of countries that make these investments possible in the first place.

The panel framework of my analysis allows me to dispel many of these worries. This is because, by using fixed effects, I control for structural differences across countries and for pre-1927 historical events. Thus, the inclusion of fixed effects eliminates the possibility that the incidence of pre-1913 conflicts could influence the magnitude of revenue changes post-1926 through channels other than fiscal capacity. The only way in which the exclusion restriction might still be violated is that a transformation in the nature of pre-1927 circumstances took place in the 12 years between 1927 and 1938 (this circumstances would have to have been caused by pre-1913 conflicts); this would, in turn, need to have an effect on the volatility of post-1927 revenues. This instance cannot be ruled out, but appears very unlikely given the short time span considered. Also, I am not aware of any study which claims such an instance. Furthermore, the inclusion of fixed effects also guarantees that pre-1913 fiscal capacity and revenue volatility are all controlled for in the regressions, since these are time-invarying country characteristics from the vantage of point of the period studied in this paper. This accounts for the potential bias coming from correlations between past capacity, past revenue volatility, past conflicts and current changes in revenues.

The over-identifying tests conducted below thanks to the combined use of historical conflicts and natural disasters further help dispel worries that warfare might not be a valid instrument These tests, however, are only valid conditional on the natural disasters variable being a relevant and valid instrument. I now turn to arguing that this is the case.

The disasters instrument

I argue that the incidence of natural disasters can affect the creation of centralized revenue raising institutions. Moreover, I argue that this effect also applies to countries outside of Europe. Thus, apart from helping dispel worries of reverse causality between conflicts and fiscal capacity outlined above, introducing an additional instrument with a broader scope presents advantages also from this point of view.

Table 4.4 offers an overview of all disasters for my sample of countries and time period contained in the EM-DAT Database of the Centre for the Epidemiology of Disasters at the Université Catholique de Louvain (CRED, 2017). The starting point of the data is 1900 because no information is available before this date. The endpoint is 1926, one year before the starting point of my analysis, in order to avoid a direct relationship between disasters and yearly changes in revenue. For an event to qualify as a disaster at least one of these conditions needs to be met: 1) $\geq$ 10 casualties, 2) $\geq$ 100 people affected, 3) declaration of a state of emergency, 4) call for international assistance. This broad definition means that the data includes natural disasters of very different magnitudes. This is reflected by the information on the number of deaths due to the disasters, which varies between 0 and millions for a single incident.

The reporting of historical disasters is clearly not as complete as that of more recent episodes. In particular, small-scale disasters in countries outside of Western Europe and North America are potentially under-reported in the dataset. Thus, the negative correlation between fiscal capacity and disasters found in the data is likely to be lower bound of the true one, given the relatively lower level of fiscal capacity found outside of Western countries. South American nations, historically characterized by low fiscal capacity, seem to be particularly underrepresented compared to their long-run disasters incidence. This measurement error could bias my results indirectly through its effect on the strength of the instrument. Standard tests, however, suggest that the instrument is strong enough to limit this potential source of bias within reasonable bounds. Robustness checks performed using the long-term disaster incidence, which is less likely to be affected by this issue, are very similar.

A potential problem with the disasters instrument is that a lower fiscal capacity might simply reflect lower tax revenues due to the direct incidence of disasters on the tax base. However, this would mean that the effect of disasters on the tax base is not captured by changes in output, which is the denominator of my fiscal capacity indicators. This is possible, but very unlikely, particularly for the income taxes capacity indicator, given

that nominal GDP is precisely a measure of income. Moreover, by analyzing a century of disasters data in the US, Boustan, Kahn, Rhode, and Yanguas (2017) show that only major disasters tend to to have significant economic repercussions. Finally, I find absolutely no direct relationship between the incidence of disasters and revenue volatility in my data.

Table 4.4: Natural disasters by country and type, 1900-26

Country	Type	Frequency	Deaths	Total disasters	Total deaths
USA	Storm	13	9,448	23	13,296
	Flood	5	713		
	Wildfire	1	1,000		
	Earthquake	4	2,135		
Japan	Volcanic activity	4	439	15	155,350
	Earthquake	7	145,332		
	Flood	1	1,379		
	Storm	3	8,000		
	Landslide	1	200		
India	Drought	1	1,250,000	11	5,794,622
	Earthquake	1	20,000		
	Epidemic	5	4,523,000		
	Storm	2	1,622		
	Flood	1	0		
Canada	Mass movement (dry)	5	245	9	50,389
	Wildfire	2	116		
	Storm	1	28		
	Epidemic	1	50,000		
Indonesia	Earthquake	4	16,147	7	26,687
	Epidemic	1	40		
	Volcanic activity	2	10,500		
Italy	Earthquake	4	107,480	6	108,280
	Volcanic activity	1	700		
	Landslide	1	100		
Romania	Earthquake	4	0	5	1,000
	Flood	1	1,000		
Belgium	Flood	3	6	3	6
Chile	Earthquake	3	21,100	3	21,100
Germany	Flood	3	0	3	0
Turkey	Earthquake	2	6,923	2	6,923
Colombia	Earthquake	1	400	2	500
	Landslide	1	100		
France	Earthquake	1	46	2	74
	Landslide	1	28		
Peru	Earthquake	1	150	1	150
New Zealand	Epidemic	1	6,700	1	6,700
Egypt	Earthquake	1	12	1	12
Total				94	6,185,089

Source: EM-DAT Database of the Centre for the Epidemiology of Disasters (CRED, 2017).

I argue that natural disasters can affect fiscal capacity in two ways. First, when resources have to be deployed towards disaster relief, all else equal, less resources can be

spent on fiscal institutions. As Besley and Persson (2010), argue, countries need to invest in order to create and strengthen these institutions, and large scale disasters can drain substantial resources, delaying or impeding these investments altogether. The devastating flood which affected around 20% of China's provinces in 1823 offers an example of this: the Qing administration devoted around half of its small (relative to the size of the economy) annual budget to disaster relief and this severely slowed down long-term investments in the provision of public goods, such as defense and infrastructure (Yuping and Uebele, 2015). Thus, disasters might divert enough resources towards relief to significantly slow down investments in fiscal institutions, particularly under conditions of low initial fiscal capacity.

Two objections can be raised against this argument. The first is that war, much like disaster relief, drains resources away from public investments; however, the historiography and this paper argue that armed conflicts can lead to the accumulation of fiscal capacity. Why should the effect of natural disasters be the opposite? A rebuttal for the first objection is that the accumulation of fiscal capacity is a long-run phenomenon. In the short-run, conflicts, like disasters, might have led to investment diversion, but in the long term they led to fiscal capacity accumulation by making fiscal reforms easier. The short time span of my disasters variable means that it overlaps with my fiscal capacity indicators, whereas the conflict data precedes the observation period. This gives the argument potential traction. However, it is ultimately unconvincing because, as Table 4.5 shows, the historical incidence of natural disasters is correlated with a lower fiscal capacity also nowadays.[47]

The second objection is that most disasters in my sample were not large enough to cause a significant displacement of resources. More than two-thirds of the total death toll for the 1900-26 period is accounted for by just three epidemics in India, and over half of the disasters led to under 500 casualties, while the mean population size of the countries in the sample is over 20 million. The second channel I propose addresses all these concerns and also offers a robust and intuitive mechanism through which disasters affect fiscal capacity.

The argument goes as follows. First, certain geographic features of countries make them more or less prone to experience natural disasters. More precisely, the interaction between land size and the presence of volcanoes, the proclivity for seismic activity, the vulnerability to storms and floods, etc. will determine the incidence of disasters in a

[47] Interestingly, Dincecco and Prado's preferred fiscal capacity indicator, the share of direct taxes in total tax revenue, is positively associated with historical disasters (but not long-run disaster proclivity) both today and in the interwar period, which once again highlights the different informational content of different fiscal capacity indicators.

country. Second, the historical disasters data is an accurate reflection of countries' average long-term vulnerability to disasters. This is easily demonstrated by the fact that the correlation between disasters in 1900-26 and 1900-90 is indeed very high (0.87) and strongly statistically significant.[48] Third, most natural disasters are small scale events affecting limited areas of a country and shares of the population. Fourth, this means that localized relief might be better suited – for example because of local level specialization in areas vulnerable to specific types of disasters – and quicker to reach the affected area compared to centrally financed and coordinated efforts. Local governments might also be held more easily accountable by citizens with regard to the provision of relief. Moreover, even large scale disaster relief undertaken by central governments relies on local level monitoring and infrastructure in order to be timely and effective, as evidenced by India's famines (Drezè and Sen, 1989; Besley and Burgess, 2002).[49] Fifth, the need to finance local level disaster relief will stimulate the accumulation of local-level fiscal capacity. Indeed, there is a positive and statistically significant correlation between the number of natural disasters and the share of taxes raised at the sub-national level in countries for which data is available in 1914-38.[50] Sixth, the local nature of disasters, of relief and of the institutions created to deal with them will make the centralization of fiscal resources more difficult in disaster-prone countries leading to the observed negative relationship between disasters and fiscal capacity.

The reasoning can also be applied from the standpoint of the citizens of areas of a country unaffected by natural disasters. These might be unwilling to mutualize resources through the central government, since part of these would fund a pubic good – disaster relief – that only benefits the citizens of the affected area and is very likely characterized by low spillovers. Under these conditions – heterogeneous preferences over public goods and low spillovers – decentralized political systems are known to outperform centralized systems (Oates, 1972; Besley and Coate, 2003). As above, this reasoning applies to recur-

[48] I pick 1990 as the endpoint of my data because in recent years there has been an increase in natural disasters potentially linked to Global Warming and exposure hazard due to population growth, and thus unrelated to the historical data generating process, see Strömberg (2007). However, if the period 1900-2015 is considered, the correlation is practically identical (0.86).

[49] In the case of large disasters, countries might also expect to be able to draw on international assistance, as was common also in the pre-WWII era, leading to the need to accumulate less fiscal capacity to provide relief.

[50] These are Austria, Belgium, Bulgaria, Switzerland, Germany, Denmark, France, UK, Hungary, Italy, Latvia, the Netherlands, Norway, Poland, Sweden, USA. The data furthermore feature a positive correlation between disasters and the share of income taxes in total tax revenue, as shown in Table 4.5. This is also compatible with the idea that disasters might stimulate the creation of local level capacity, making it easier for local governments to raise direct taxes. This, in turn, may also open the way for centralized direct taxation, but not high overall levels of taxation.

ring disasters due to a region's geographical characteristics, which make it more vulnerable to certain types of events, rather than to idiosyncratic occurrences of disasters.

Table 4.5: Correlation between contemporary and historical fiscal capacity indicators and instruments

		1914-26 averages			1990s averages		Instruments	
		Tax/GDP	IncomeTax/GDP	IncomeTaxShare	Tax/GDP	IncomeTaxShare	YearsAtWar	NatDisasters
1914-26 averages	Tax/GDP	1						
	IncomeTax/GDP	0.799***	1					
		(0.000)						
	IncomeTaxShare	0.218	0.675***	1				
		(0.222)	(0.000)					
1990s averages	Tax/GDP	0.546***	0.519****	0.367**	1			
		(0.001)	(0.002)	(0.026)				
	DirectTaxShare	0.002	0.125	0.399**	0.350**	1		
		(0.992)	(0.489)	(0.013)	(0.027)			
Instruments	1816-1913 YearsAtWar	0.310*	0.576***	0.446**	0.129	0.1560	1	
		(0.085)	(0.006)	(0.056)	(0.436)	(0.325)		
	1900-26 NatDisasters	-0.332*	-0.149	0.381**	-0.318**	0.326**	0.001	1
		(0.059)	(0.409)	(0.018)	(0.045)	(0.037)	(0.998)	
	1900-90 NatDisasters	-0.493***	-0.275	0.271*	-0.461***	0.157	0.143	0.868***
		(0.009)	(0.121)	(0.099)	(0.003)	(0.328)	(0.381)	(0.000)

*** p<0.01, ** p<0.05, * p<0.1

Significance levels in brackets. Source: The 1990s indicators and the conflict data are from Dincecco and Prado (2012); the natural disasters data is from the EM-DAT Database of the Centre for the Epidemiology of Disasters (CRED, 2017); for the historical indicators see text.

An obvious rebuttal of this argument is the possibility of reverse causality: countries might experience less natural disasters thanks to better infrastructure and other means of disaster prevention made possible by a higher level of fiscal capacity. However, the consensus in the literature appears to be that "good institutions", and economic development more generally, strongly affect the severity of the consequences of disasters, but not their frequency (Kahn, 2005; Strömberg, 2007; Lin, 2015). In other words, disasters are indeed *natural*, in the sense that they are caused by geographical features, but there is ample scope for policies to mitigate their death tolls, the number of people hey affect and the material damage they create.

4.5 Results

Table 4.6 illustrates the elasticity of tax revenues and government financing to changes in nominal GDP for 1927-38. The first two columns feature OLS estimations for tax revenues and government financing, while columns 3 and 4 contain the within (FE) estimations and columns 5 and 6 present first differences (FD) results. As expected, government revenues and the nominal GDP strongly co-move. The key result of the exercise, however, is that the coefficient is close to 1 when tax revenues are considered, but becomes slightly more than

half of that when government financing is the outcome variable. These results indicate the existence of a strong smoothing effect of non-tax revenues.

In the table, I only use observations for which both financing and tax revenues data are available, thus the result is not due to differences in the composition of the financing and tax revenues samples. I have repeated the exercise for 1927-38 and for an extended time period (1920-38) using all observations available and obtained remarkably similar results (see Appendix 4.A). This indicates that the smoothing role of non-tax revenues was important and constant throughout the whole interwar period.

The rest of the analysis in this section shows that this smoothing effect is due to the degree of fiscal capacity countries possessed at the eve of the Great Depression. In Section 4.6, I dig deeper into the channels, showing that fiscal capacity granted an easier access to borrowing to more fiscally capable countries.

Table 4.6: The elasticity of tax revenues and government financing to changes in output

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	FE	FE	FD	FD
VARIABLES	ΔTaxRevenue	ΔGovtFinancing	ΔTaxRevenue	ΔGovtFinancing	ΔTaxRevenue	ΔGovtFinancing
ΔNGDP	0.929***	0.563***	1.012***	0.617***	0.875**	0.495***
	(0.241)	(0.124)	(0.352)	(0.132)	(0.407)	(0.176)
Constant	0.0429	0.0288***	0.0430***	0.0289***	0.00242	-0.00337
	(0.0277)	(0.00670)	(0.000161)	(6.01e-05)	(0.00738)	(0.00837)
Country FE			✓	✓	✓	✓
Observations	239	239	239	239	199	199
Number of countries	32	32	32	32	32	32
R-squared	0.034	0.101	0.039	0.101	0.012	0.044
Robust standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

All changes (Δ) are percentage changes. Time frame: 1927-38. Countries included are: Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Greece, Hungary, India, Ireland, Italy, Japan, the Netherlands, Norway, Poland, Romania, South Africa, Spain, Sweden, Switzerland, Turkey, the United Kingdom, the United States of America and Yugoslavia.

4.5.1 OLS results

I continue the analysis by introducing the main variables of interest – the two fiscal capacity indicators – alongside the controls and by adopting the interaction model described by equation 4.3. Table 4.7 reports the pooled OLS estimation results. In columns 1-4, I control for the Polity 2 score from the POLITY IV database (Marshall and Jaggers, 2005) – to capture potential differences between more or less democratic countries – and for GDP per capita (GDPPC) from the latest version of the Maddison Project database

(Bolt and van Zanden, 2013), to capture differences between more and less economically advanced countries. Both controls are extremely important due to the fact that the level of democracy and economic development are likely correlated with both fiscal development and changes in revenue. I repeat the exercise in columns 5-8, adding the structure of tax revenues as a control. Controlling for the source of tax revenues is important in order to identify the effect of fiscal capacity because a different degree of revenue cyclicality could be connected to a different reliance on the various tax bases, which could, in turn, be related to the level of development of the tax system. As discussed above, I enter all these variables as 1914-26 averages. I have experimented by adding other controls – such as gold standard membership, trade openness, the size and composition of the public debt, and the terms of trade – finding practically identical results throughout the analysis.

While these simple OLS estimates clearly suffer from two potential sources of bias – omitted variables and reverse causality – which rule out any causal interpretation of the coefficients, they nonetheless illustrate some informative correlations. Coherently with the interpretation of the elasticity results outlined above, once I introduce the fiscal capacity indicators and the controls in the regressions, the responsiveness of government financing and tax revenues to changes in economic activity becomes very similar. This strongly suggests that the variables in the regressions fully capture the smoothing effect of non-tax government revenues.

In all specification the interaction term between the fiscal capacity measure and NGDP comes out as strongly significant and negative. This indicates a countercyclical association between fiscal capacity and changes in both tax revenues and government financing. While the rest of the analysis confirms the latter, it indicates that the former might be simply due to bias. In any case, the relationship also appears to be non-linear in the size of the change in NGDP, meaning that it is stronger for larger changes in NGDP.

4.5.2 Fixed effects results

An important step towards a causal interpretation of the results is to include country fixed-effects in the regressions. As discussed above, these account for the myriad of potentially important time-invarying and slow-moving country characteristics both observable and not. Examples include geography, demographic and economic structure, the presence of natural resources and past success or failure in wars (this is important for the IV estimation below). I also include time fixed-effects to account for common shocks across countries.

Table 4.8 and Table 4.9 present the results of the panel estimations for tax revenues

Table 4.7: Pooled OLS estimation for tax revenues and government financing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	ΔTaxRevenue	ΔTaxRevenue	ΔGovtFinancing	ΔGovtFinancing	ΔTaxRevenue	ΔTaxRevenue	ΔGovtFinancing	ΔGovtFinancing
ΔNGDP	1.440***	0.941***	1.899***	0.965**	2.100***	1.300***	2.254***	1.057*
	(0.416)	(0.317)	(0.525)	(0.381)	(0.514)	(0.397)	(0.681)	(0.511)
Tax/NGDP	-0.217		-0.376**		-0.129		-0.481**	
	(0.176)		(0.165)		(0.180)		(0.175)	
ΔNGDP*Tax/NGDP	-8.498***		-13.51***		-10.21***		-14.53***	
	(3.008)		(4.089)		(2.388)		(4.752)	
IncomeTax		-0.272		-0.283		-0.0952		-0.563***
		(0.231)		(0.247)		(0.274)		(0.152)
ΔNGDP*IncomeTax		-13.96***		-16.51**		-18.27***		-20.32**
		(3.529)		(6.249)		(2.283)		(9.400)
Constant	0.0383*	0.0286**	0.0391**	0.0182	0.0141	0.0110	0.0362*	0.00942
	(0.0201)	(0.0112)	(0.0164)	(0.0118)	(0.0207)	(0.0132)	(0.0184)	(0.0221)
Additional controls								
Polity2	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*Polity2	✓	✓	✓	✓	✓	✓	✓	✓
GDPPC	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*GDPPC	✓	✓	✓	✓	✓	✓	✓	✓
IncomeTaxShare					✓	✓	✓	✓
ΔNGDP*IncomeTaxShare					✓	✓	✓	✓
IndirectTaxShare					✓	✓	✓	✓
ΔNGDP*IndirectTaxShare					✓	✓	✓	✓
Observations	208	208	219	219	208	208	219	219
R-squared	0.307	0.306	0.165	0.145	0.326	0.322	0.171	0.149
Robust standard errors in parentheses								
*** p<0.01, ** p<0.05, * p<0.1								

All changes (Δ) are percentage changes. Time frame: 1927-38. Countries included: Argentina, Australia, Austria, Belgium, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, South Africa, Spain, Sweden, Switzerland, the United Kingdom and the United States.

and government financing respectively. In both tables, columns 1 and 2 provide the FE estimates and columns 3 and 4 feature the FD results. Both estimators require time-invarying regressors to be dropped. Columns 5 and 6 present the results of the HT (Hausman and Taylor, 1981) estimator, which allows me to recover the coefficients of time-invarying regressors even in the presence of fixed effects.

The panel results for tax revenues largely confirm the statistically significant (in all specifications but one, column 4), sizable and non-linear countercyclical effect of fiscal capacity. This is evidenced by the negative coefficients on the interaction term between the fiscal capacity indicators – Tax/NGDP and IncomeTax/NGDP – and the change in nominal GDP. The effect is somewhat smaller than in the pooled OLS estimations, which suggests that the fixed effects account for some bias. As the IV analysis shows, the panel estimation is insufficient to account for all bias. Once this is done, the significant effect of fiscal capacity on tax revenue volatility disappears.

The results for government financing are stronger and more clear cut. The interaction term between the fiscal capacity indicators and changes in GDP is statistically significant, negative and close to the pooled OLS estimates. The statistically significant negative coefficients of the non-interacted fiscal capacity indicators obtained with the HT estima-

Table 4.8: Panel estimation for tax revenues

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	FE	FD	FD	HT	HT
VARIABLES	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue
ΔNGDP	1.835***	1.042**	0.644	0.375	1.762***	1.038***
	(0.586)	(0.449)	(1.000)	(0.692)	(0.516)	(0.377)
Tax/NGDP					-0.163	
					(0.163)	
ΔNGDP*Tax/NGDP	-9.822***		-3.785		-9.309***	
	(3.182)		(7.289)		(3.063)	
IncomeTax/NGDP						-0.187
						(0.253)
ΔNGDP*IncomeTax/NGDP		-19.24***		-14.44*		-18.74***
		(3.320)		(8.303)		(3.111)
Constant	0.153***	0.156***	-0.00806	-0.00828	-0.0517	-0.0611
	(0.0463)	(0.0464)	(0.0564)	(0.0559)	(0.0426)	(0.0391)
Additional controls						
Polity2					✓	✓
ΔNGDP*Polity2	✓	✓	✓	✓	✓	✓
GDPPC					✓	✓
ΔNGDP*GDPPC	✓	✓	✓	✓	✓	✓
IncomeTaxShare					✓	✓
ΔNGDP*IncomeTaxShare	✓	✓	✓	✓	✓	✓
IndirectTaxShare					✓	✓
ΔNGDP*IndirectTaxShare	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	208	208	184	184	208	208
Number of countries	23	23	23	23	23	23
R-squared	0.373	0.375	0.126	0.132		
Robust standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

All changes (Δ) are percentage changes. Time frame: 1927-38. Countries included: Argentina, Australia, Austria, Belgium, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, South Africa, Spain, Sweden, Switzerland, the United Kingdom and the United States.

tor further suggest that the interaction term coefficients represent a lower bound of the smoothing effect of fiscal capacity on government financing. The non-linearity of the effect of fiscal capacity in the size of the NGDP shock is also evident: the smoothing effect of fiscal capacity increases as the size of the output shock increases. In summary, the countercyclical effect of fiscal capacity on government financing, however measured, is clearly visible and very sizable.

4.5.3 IV Results

I start my IV analysis by running pooled regressions, which I report in Appendix 4.A. These indicate that the smoothing effect of fiscal capacity is to be found in non-tax rev-

Table 4.9: Panel estimation for government financing

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	FE	FD	FD	HT	HT
VARIABLES	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing
ΔNGDP	2.053***	1.015**	1.049**	0.233	2.012***	0.953**
	(0.505)	(0.420)	(0.487)	(0.387)	(0.530)	(0.433)
Tax/NGDP					-0.506***	
					(0.172)	
ΔNGDP*Tax/NGDP	-12.62***		-10.14***		-12.86***	
	(3.625)		(2.596)		(3.822)	
IncomeTax/NGDP						-0.828***
						(0.292)
ΔNGDP*IncomeTax/NGDP		-19.71**		-15.15***		-19.84**
		(8.219)		(4.652)		(8.698)
Constant	0.150***	0.156***	0.0628	0.0632	0.0121	-0.0191
	(0.0287)	(0.0303)	(0.0423)	(0.0426)	(0.0265)	(0.0265)
Additional controls						
Polity2					✓	✓
ΔNGDP*Polity2	✓	✓	✓	✓	✓	✓
GDPPC					✓	✓
ΔNGDP*GDPPC	✓	✓	✓	✓	✓	✓
IncomeTaxShare					✓	✓
ΔNGDP*IncomeTaxShare	✓	✓	✓	✓	✓	✓
IndirectTaxShare					✓	✓
ΔNGDP*IndirectTaxShare	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	219	219	210	210	219	219
Number of countries	23	23	23	23	23	23
R-squared	0.244	0.234	0.108	0.103		
	Robust standard errors in parentheses					
	*** p<0.01, ** p<0.05, * p<0.1					

All changes (Δ) are percentage changes. Time frame: 1927-28. Countries included: Argentina, Australia, Austria, Belgium, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, South Africa, Spain, Sweden, Switzerland, the United Kingdom and the United States.

enues: for tax revenues the effect of fiscal capacity appears to be indistinguishable from zero, while for government financing it is once again statistically significant and negative. The results also provide further evidence that the interaction term coefficients offer a lower bound of the true smoothing effect of fiscal capacity.

Table 4.10 features the panel IV results for tax revenues and Table 4.11 for government financing. In order to explore the robustness of the results, I run the regressions employing the instruments both one at a time and in conjunction. In both Tables, columns 1-4 present the FE and FD estimates using the warfare instrument, columns 5 to 8 contain the results for the natural disasters instrument and columns 9 to 12 feature the instruments used in conjunction.[51]

[51] It should be noted that in the IV estimations I use 1914-38 averages as fiscal capacity, rather than the 1914-26 averages used above, in order to decrease concerns over bias due to weak instruments. The 1914-38 averages have a higher correlation with the instruments which, incidentally, also supports the idea that

In the two-instrument set-up, I use the Fuller-k estimator due to its greater robustness to the presence of weak instruments and the inclusion of multiple instruments (Stock, Wright, and Yogo, 2002; Stock and Yogo, 2005). The Fuller-k, estimator is inconsistent in the presence of heteroskedasticity and many instruments (Hausman, Lewis, Menzel, and Newey, 2011), and although the many instruments condition is not met in this application, I perform robustness checks with both the two stage least squares (2SLS) and Continuously Updated Generalized Method of Moments estimator (CUE). The latter, in particular, is consistent when errors are non-normal in the presence of many instruments. Both estimators yield very similar results to the Fuller-k. When the equation is exactly identified (one instrument and one endogenous regressor), all these estimators are equivalent.

The critical F-stat values for robust estimation for the Fuller-k estimator with two instruments for a maximal 5% and 10% bias over OLS are 15.5 and 12.55 respectively (Stock and Yogo, 2005). These are met for Tax/NGDP, but weak instruments are a potential issue for the estimations using the IncomeTax/NGDP fiscal capacity indicator. However, the fact that these results are similar to the results for the Tax/Revenue indicator throughout the whole analysis and in the Panel IV framework is very reassuring.

The results in Table 4.10 illustrate the usefulness of the IV approach: the coefficients of the interacted fiscal capacity indicators are much smaller than in the standard panel framework, and statistically indistinguishable from zero in all specifications except one (FE using natural disasters as instrument). When I use the instruments in conjunction and the the more robust FD estimator, the coefficient turns positive (albeit still insignificant) possibly indicating a pro-cyclical rather than countercyclical effect of fiscal capacity on tax revenues. In terms of fiscal policy, this would mean that high fiscal capacity countries had more procyclical tax revenues, which would be in line with the prescriptions of both Keynesian and non-Keynesian models of fiscal policy. In any case, the general conclusion is that no robust causal impact of fiscal capacity on tax revenues emerges from the analysis. This indicates that the IV approach is correcting a source of bias that links a high tax procyclicality to low fiscal capacity. A compelling possibility is reverse causality: high tax revenue volatility might have made investments in fiscal capacity more difficult.

The results in Table 4.11, instead are clear-cut, large and highly statistically significant. In the interwar period, fiscal capacity had a strong smoothing effect on government financing, and this was evidently due to non-tax revenues. Moreover, this effect was highly

fiscal capacity develops gradually over the medium-long term. Given that I use instrumental variables, the simultaneity between changes in revenues and capacity which was concern in the estimations above is lo longer an issue. This is as also confirmed by the fact that the estimates are practically identical qualitatively and quantitatively when the 1914-26 indicators are employed in the IV framework.

non-linear and increased in magnitude with the size of the economic shock. As I will show below, borrowing is likely to have played the dominant role in determining this effect. The results are also confirmed when I instrument changes in output using the average change in nominal GDP of trading partners weighted by pre-Depression trade shares (see Appendix 4.A).

Two further robust results emerge from the estimation. On one hand, countries with a higher per capita GDP were able to smooth their government financing more effectively than lower income countries. On the other hand, countries with more democratic institutions were subject to a higher volatility in government financing. Exploring these results further is beyond the scope of this paper, but one can reasonably speculate that richer countries found it easier to borrow on financial markets and thus smooth their revenues, as also confirmed by the analysis in Section 4.6. More democratic countries, instead, might have found it more challenging to borrow due to the political constraints and delays involved in a parliamentary budgeting process. Empirical evidence on the link between democracy and fiscal policy procyclicality is mixed. By analyzing OECD countries, Lane (2003) finds a positive link between the dispersion of political power and fiscal policy procyclicality, but evidence from a country transitioning from autocracy to democracy – Spain – demonstrates that the relationship between democratic institutions and the cyclicality of policy is potentially more intricate (Battilossi, Escario, and Foreman-Peck, 2013).

In the interwar context, less democratic countries might have also found it easier to engage in financial repression, with the aim of channeling resources towards government borrowing. This was notoriously the case in Nazi Germany, for example (Poole, 1939; Childs, 1958). The crucial point, however, is that the effects of fiscal capacity, economic development and democratic institutions on the smoothing of government financing can be identified separately, suggesting different underlying channels.

Table 4.10: Panel instrumental variable estimation for tax revenues

	Panel A: Second Stage											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	FE	FE	FD	FD	FE	FE	FD	FD	Fuller-k FE	Fuller-k FE	Fuller-k FD	Fuller-k FD
VARIABLES	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue	ΔTaxRevenue
ΔNGDP	1.173	0.993**	-0.199	0.251	2.150***	1.288***	0.655	0.363	1.519**	1.078**	0.0682	0.282
	(0.832)	(0.504)	(1.049)	(0.756)	(0.622)	(0.487)	(0.825)	(0.659)	(0.700)	(0.466)	(0.956)	(0.717)
ΔNGDP*Tax/NGDP	-3.200		9.255		-15.78***		-6.039		-7.662		4.474	
	(8.256)		(12.41)		(5.143)		(7.320)		(6.616)		(10.76)	
ΔNGDP*IncomeTax/NGDP		-6.552		19.67		-34.62***		-13.46		-14.60		10.62
		(15.94)		(29.69)		(12.23)		(14.87)		(12.61)		(25.17)
Constant			0.00477	0.00529			0.00480	0.00445			0.00478	0.00506
			(0.00688)	(0.00676)			(0.00693)	(0.00710)			(0.00688)	(0.00682)
Additional controls												
ΔNGDP*Polity2	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*GDPPC	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*IncomeTaxShare	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*IndirectTaxShare	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	208	208	184	184	208	208	184	184	208	208	184	184
Number of countries	23	23	23	23	23	23	23	23	23	23	23	23
R-squared	0.358	0.361	0.118	0.101	0.336	0.349	0.116	0.126	0.358	0.366	0.124	0.115
	Panel B: First stage											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ΔNGDP*YearsAtWar	0.000620***	0.000303**	0.000606***	0.000285***					0.000475***	0.00024***	0.000493***	0.000237***
	(0.000144)	(0.000110)	(0.000118)	(0.000100)					(0.0001044)	(0.0000854)	(0.000078)	(0.0000772)
ΔNGDP*NatDisastNumb					-0.00285***	-0.001300**	-0.00290***	-0.00130**	-0.00162**	-0.000668**	-0.00172***	-0.000735**
					(0.000970)	(0.000613)	(0.000922)	(0.000598)	(0.000655)	(0.000314)	(0.000597)	(0.000313)
Angrist-Pischke F-Stat	18.57***	7.52**	26.40***	8.12***	8.64***	4.49**	9.92***	4.75**	15.97***	5.21**	31.51***	6.18***
Hansen J-Stat									3.207*	4.242**	3.241*	3.068*
Endogeneity test	0.276	0.925	1.718	3.279*	5.767**	4.230*	2.606	0.162	2.303	0.827	0.101	0.538
Robust standard errors in parentheses												
*** p<0.01, ** p<0.05, * p<0.1												

All changes (Δ) are percentage changes. Time frame: 1927-28. Countries included: Argentina, Australia, Austria, Belgium, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, South Africa, Spain, Sweden, Switzerland, the United Kingdom and the United States.

Table 4.11: Panel instrumental variable estimation for government financing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Panel A: Second Stage											
	FE	FE	FD	FD	FE	FE	FD	FD	Fuller-k FE	Fuller-k FE	Fuller-k FD	Fuller-k FD
VARIABLES	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing
ΔNGDP	2.000***	1.226***	0.675	0.299	2.663**	1.450**	1.238***	0.446	2.206***	1.290***	0.833**	0.343
	(0.619)	(0.439)	(0.453)	(0.370	(1.034)	(0.625)	(0.454)	(0.370)	(0.721)	(0.482)	(0.418)	(0.356)
ΔNGDP*Tax/NGDP	-13.55**		-6.781**		-21.52**		-14.14***		-16.02***		-8.841***	
	(5.273)		(3.054)		(9.470)		(3.827)		(6.140)		(2.727)	
ΔNGDP*IncomeTax/NGDP		-28.62**		-14.25**		-46.74*		-29.01***		-33.80**		-18.72***
		(12.58)		(5.756)		(24.56)		(10.83)		(15.38)		(5.944)
Constant			0.00553	0.00544			0.00607	0.00587			0.00568	0.00557
			(0.00444)	(0.00437)			(0.00444)	(0.00428)			(0.00444)	(0.00435)
	Additional controls											
ΔNGDP*Polity2	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*GDPPC	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*IncomeTaxShare	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*IndirectTaxShare	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	219	219	210	210	219	219	210	210	219	219	210	210
Number of countries	23	23	23	23	23	23	23	23	23	23	23	23
R-squared	0.233	0.227	0.103	0.101	0.212	0.202	0.099	0.095	0.229	0.222	0.103	0.100
	Panel B: First stage											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ΔNGDP*YearsAtWar	0.000668***	0.000316***	0.000683***	0.000325***					0.000521***	0.000252***	0.000542***	0.000253***
	(0.0001485)	(0.000112)	(0.0000389)	(0.000106)					(0.000119)	(0.0000876)	(0.000088)	(0.0000782)
ΔNGDP*NatDisastNumb					-0.00283***	-0.00130**	-0.00301***	-.00147**	-0.00143**	-0.000627**	-0.00142**	-0.000725**
					(0.00092)	(0.000603)	(0.000929)	(0.000617)	(0.000589)	(0.000295)	(0.000543)	(0.000323)
Angrist-Pischke F-Stat	20.22***	7.98***	29.51***	9.38***	9.47***	4.67**	10.51***	5.66**	16.88***	5.36**	28.63***	7.20***
Hansen J-Stat									1.561	1.965	2.715*	2.785*
Endogeneity Test	0.944	1.384	0.264	0.005	2.301	2.710*	2.391	4.322**	0.546	0.711	0.057	1.634
	Robust standard errors in parentheses											
	*** p<0.01, ** p<0.05, * p<0.1											

All changes (Δ) are percentage changes. Time frame: 1927-28. Countries included: Argentina, Australia, Austria, Belgium, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, South Africa, Spain, Sweden, Switzerland, the United Kingdom and the United States.

4.5.4 Quantitative impact

This section offers an overview of the economic magnitude of the findings. I first report the marginal effects of the variables of interest as well as some concrete examples of the impact of fiscal capacity under various assumptions. I then offer some admittedly naive, but suggestive counterfactuals through which I illustrate the path of revenues over the course of the Great Depression in a number of countries by assigning them different levels of fiscal capacity.

Summary statistics and marginal effects

Table 4.12 presents the summary statistics of the dependent variables and the principal regressors employed in the estimations above. The range of values taken by both sets of variables is large and offers much scope for investigating the quantitative effects of fiscal capacity on government financing. For the rest of this section, I consider only negative changes in NGDP for illustrative purposes, but the effects for positive changes are symmetric.

Table 4.12: Summary statistics of the principal variables

	1st Percentile	1st Quartile	Mean	Median	3rd Quartile	99th Percentile
Δ TaxRevenue	-32.8	-3.3	4.9	2.9	8.9	42.2
Δ TaxRevenue (< 0)	-41.5	-12.3	-8.8	–5.6	-2.2	-0.2
Δ GovtFinancing	-36.1	-4.3	2.3	2.1	8.3	59.7
Δ GovtFinancing (< 0)	-37.7	-13.7	-10.1	-7.4	-2.8	-0.1
Δ NGDP	-25.6	-3.4	1.3	2.4	6.6	25
Δ NGDP (< 0)	-30.5	-11	-7.4	-5.6	-2.2	-0.1
Tax/NGDP (%)	1.5	5.0	6.5	6.2	8.0	14.4
IncomeTax/NGDP (%)	0.2	1.1	2.1	2.0	2.6	8.5

All changes (Δ) are percentage changes. The fiscal capacity indicators are 1914-26 averages, while ΔNGDP, Δ TaxRevenue and Δ GovtFinancing are for the 1927-38 period.

Table 4.13 presents the marginal effects of the fiscal capacity indicators on government financing. I computed these using the HT and Panel IV coefficients obtained employing the two instruments in conjunction. The HT coefficients represent the more conservative estimate, given that the IV estimates are almost always significantly larger. Due to the nonlinearity of the effect of fiscal capacity, I calculate marginal effects for different changes in output. More precisely, I use the median negative change in NGDP in the acute Great Depression phase (-8.5%) and two other values well within the observed range (-5% and -15%). The nonlinearity emerges very strongly from the exercise: the HT marginal effect is

around four times larger for an income shock of -8.5% compared to -5% for the Tax/NGDP fiscal capacity indicator and six times larger for the IncomeTax/NGDP indicator. The marginal effect grows by around another two and a half times when the GDP shock is -15%. The nonlinearity is less pronounced for the Panel IV results because the coefficient of the non interacted fiscal capacity indicators is absent. Nonetheless, the coefficient grows by more than one and a half times for both indicators as the magnitude of the shock to GDP becomes larger.

Table 4.13: Marginal effects of fiscal capacity on government financing for different magnitudes of contractions in nominal GDP

		ΔNGDP= -5.0%	ΔNGDP= -8.50%	ΔNGDP= -15.0%
HT	Tax/NGDP	0.009	0.038	0.093
	IncomeTax/NGDP	0.003	0.018	0.044
Panel IV	Tax/NGDP	0.029	0.049	0.086
	IncomeTax/NGDP	0.019	0.033	0.058
IV/HT	Tax/NGDP	3.229	1.28	0.932
	IncomeTax/NGDP	5.691	1.853	1.307

The Panel IV coefficents are those obtained used both instruments in conjunction. The fiscal capacity indicators take their average values for 1914-26: Tax/NGDP=6.5%, IncomeTax/NGDP=2.07%. The effects are rounded up to three decimal points.

I provide a clearer overview of the magnitude of the effect of fiscal capacity in Table 4.14. I use the same changes in NGDP as above and average values for the other regressors to illustrate how the predicted change in government financing mutates with different degrees of fiscal capacity. For a -8.5% income shock, bringing a country from the 1st quartile to the 3rd quartile of fiscal capacity reduces the predicted fall in government financing by almost a quarter. The effect is stronger – a nearly 40% reduction in the decrease of financing – for a fall in NGDP of 15%. For levels of fiscal capacity near the right tail of the distribution, the predicted change in government financing turns positive. This suggests that high capacity countries would have been able not only to limit the procyclicality of fiscal policy, but to run potentially countercyclical fiscal policies.

Some (naive) counterfactuals

Counterfactuals offer an even more straightforward way to grasp the size of the effect of fiscal capacity on government financing. These compare countries' predicted government financing paths based on different fiscal capacity scenarios to their actual paths. The analysis of this paper is not structural, so fully fledged counterfactuals are simply not obtainable. However, the examples below are both intuitive and suggestive as to how much

Table 4.14: Predicted changes in government financing for different levels of fiscal capacity and changes in NGDP

Tax/NGDP	Predicted ΔGovtFinancing	IncomeTax/NGDP	Predicted ΔGovtFinancing
		ΔNGDP=-5%	
smallest	-6%	smallest	-0.6%
1st quartile	-5.5%	1st quartile	-0.5%
median	-5.3%	median	-0.3%
3rd quartile	-5.1%	3rd quartile	-0.2%
Largest	-4.2%	Largest	0.7%
		ΔNGDP=-8.5%	
smallest	-9.6%	smallest	-1%
1st quartile	-7.6%	1st quartile	-0.1%
median	-6.8%	median	0.6%
3rd quartile	-5.8%	3rd quartile	1.1%
Largest	-2%	Largest	6.2%
		ΔNGDP=-15%	
smallest	-16.3%	smallest	-1.6%
1st quartile	-11.5%	1st quartile	0.5%
median	-9.6%	median	2.3%
3rd quartile	-7.2%	3rd quartile	3.7%
Largest	2%	Largest	16.3%

The coefficients used correspond to the HT estimates. For Tax/NGDP, smallest=1.53%, 1st quartile=4.95%, median=6.24% 3rd quartile= 7.95% and largest= 14.4%. for IncomeTax/NGDP , smallest=0.17%, 1st quartile=1.12%, median=1.96% 3rd quartile= 2.6% and largest= 8.47%.

the path of government financing was influenced by the level of fiscal capacity countries possessed on the eve of the Great Depression. I use the more conservative HT coefficients and the Tax/NGDP indicator only in my calculations for synthesis. Results are similar using the income tax indicator and alternative coefficients.

While going through the counterfactuals, it useful to recall what the optimal path of fiscal aggregates over the business cycle would look like according to standard economic theory. Both in a Keynesian and tax-smoothing framework tax revenues should fall during recessions. In the tax-smoothing world, tax revenues should not change as a share of GDP over the business cycle, while in the Keynesian world they can fall further than output to stimulate aggregate demand. In both cases, this would imply counter-cyclical borrowing to smooth government financing and fund the acyclical, or counter-cyclical – here the tax-smoothing and Keynesian doctrines potentially differ again – government expenditure (Barro, 1979; Lucas and Stokey, 1983; De Long and Summers, 1986).

Recalling a broader view of public revenue and expenditure is also useful. In countries where states collect low levels of revenues as a share of GDP, sharp falls in public financing

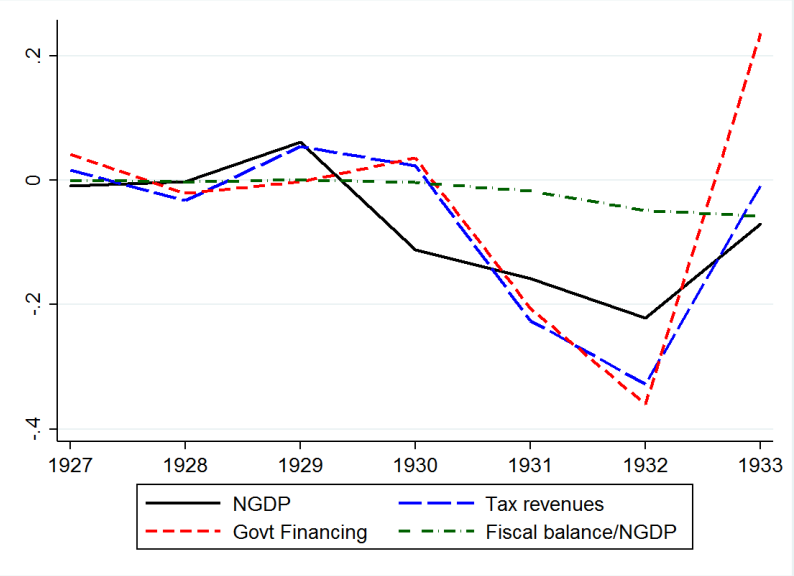
and expenditure can endanger the very ability of states to function and provide basic public goods. This can have potentially severe economic and political consequences in both the short and long run. Moreover, fiscal policy volatility and procyclicality can, both directly and to the extent that they contributes to macroeconomic volatility, also damage a country's growth prospects (see discussion in Section 4.2). In summary, the optimal path of government financing should be less procyclical than that of tax revenues – or even countercyclical – in order to, at the very least, not exacerbate business cycle volatility or affect the functioning of the state infrastructure.

The first example I provide is that of the United States (Figure 4.3). The US was one of the worst hit countries in the Great Depression, with nominal GDP contracting by approximately 46% and real GDP per capita by 31% between 1929 and 1933. Government financing and tax revenues followed a similar path, contracting cumulatively by 47% and 35% respectively between 1929 and 1933.

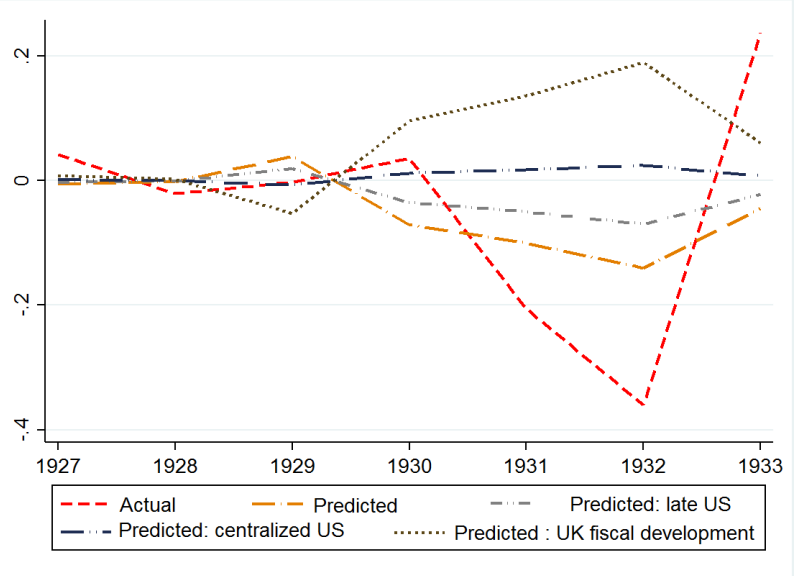
The path of US government financing was thus markedly pro-cyclical during the worst years of the Great Depression and non-tax revenues provided no smoothing. What prevented an outright collapse in spending was an increase in short-term borrowing, which led to the general fiscal policy stance being essentially neutral. Fiscal deficits reached less than 6% of GDP by 1933 and were, for the most part, the result of budgetary planning errors (De Long, 1998), thus the need to resort to short-term sources of finance. By comparison, the fiscal deficit reached 10% of GDP in 2009, while the cumulative contraction in real GDP per capita between 2007 and 2009 was less than one sixth of the interwar one, around 5% compared to 31%.

The New Deal introduced by the newly elected Franklin D. Roosevelt in 1933 led to the implementation of a number of new public expenditure programs. The policies aimed at combating the Depression also paved the way for a dramatic expansion of the Federal Government, and not just on the expenditure side (Wallis and Weingast, 2005; Fishback and Wallis, 2013).[52] For instance, the income tax – individual, corporate and payroll – was strengthened at both national and state level and became the main source of tax revenue. While national income tax collection fell in 1929-33, it rose for the rest of the decade and shot up during WWII with the reduction of personal deductions, increases in marginal rates and the beginning of withholding (Wallis, 2000). Thus, a centralization and expansion of tax revenue collection – in other words, an expansion of fiscal capacity

[52]Some economic programs were funded by national sources and administered by sub-national bodies, while the national system of defense and old age was run by the central government. National collection and federal expenditure became standard for education, highways, water and sewage, and public welfare (Wallis, 2000).



(a) Dynamics of economic activity and fiscal aggregates



(b) Government financing counterfactuals

Figure 4.3: US counterfactual, 1927-33

The NGDP, government financing and tax revenues are annual percentage changes. The fiscal balance a share of GDP is in levels. The counterfactuals are based on the HT model for the TAX/NGDP fiscal capacity indicator for 1914-26

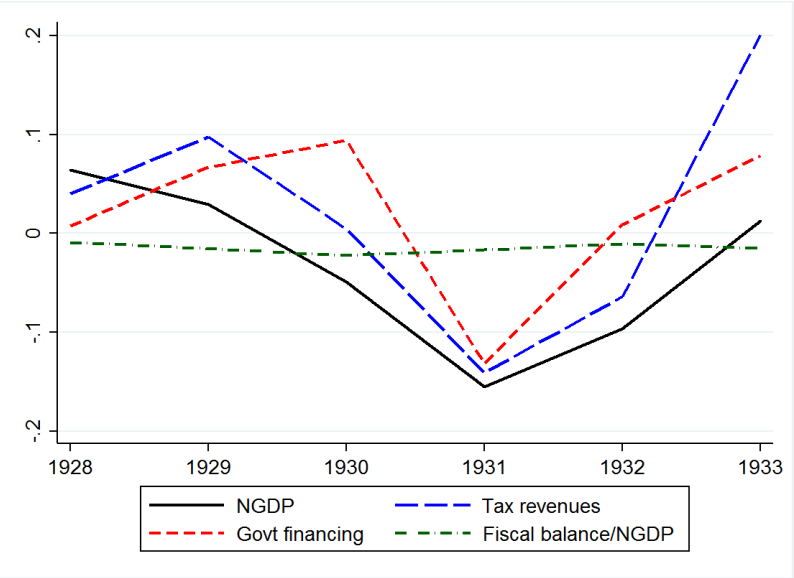
– accompanied the introduction of new policies on the expenditure side.

Given the average response of government financing to changes in NGDP embedded in my estimates, how different could the path of financing, and therefore fiscal policy, have looked like during the slump had the US entered the Great Depression with a different level of fiscal capacity? As a first first counterfactual, I assign to the US government in 1927 the fiscal capacity it had towards the end of my period of analysis, more precisely in 1937, when the drastic centralization of fiscal resources and expansion in income taxation

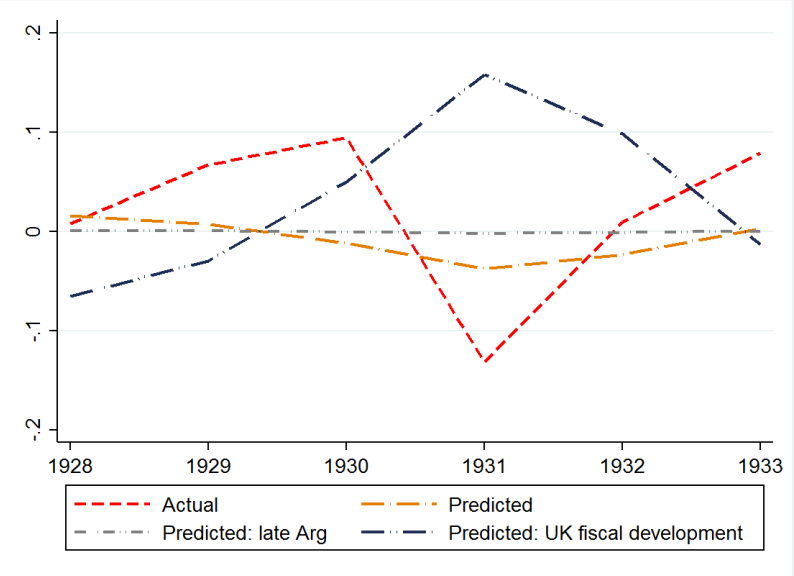
described was already well under way. The result is a significant reduction in the fall of government financing in the Depression years and a general decrease in its procyclicality. In the second counterfactual, I transform the US into a fiscally centralized country by assigning the fiscal capacity of local governments (states and municipalities) to the federal government. The result is to render financing essentially acyclical. The final counterfactual assigns to the US federal government the fiscal capacity of the United Kingdom, the most fiscally capable nation in my sample. In this case, government financing becomes markedly counter-cyclical. Therefore, given even the extremely underwhelming average fiscal policy response to the Great Depression embedded in my estimates, the United States might have been able to run a less pro-cyclical and even a counter-cyclical fiscal policy in the acute Great Depression years had it possessed a higher level of fiscal capacity on the eve of the slump, potentially easing some of its pain.

The counterfactual for Argentina yields similar lessons (Figure 4.4), but with some interesting differences. Argentina, like the US, was hit hard by the Depression. Nominal GDP contacted by nearly 27% and real GDP per capita by 17%. The external sector was particularly badly hit with trade as a share of GDP falling by more than one third. Tax revenues and government financing both collapsed with economic activity, so that when the Argentinean provisional government took office after the September Revolution of 1930, revenues were insufficient to cover the government's expenses (Alhadeff, 1985). In fact, public expenditure fell in unison with revenues. Fiscal policy was thus completely passive with a fiscal deficit close to zero: neither short nor long-term borrowing played a role in the smoothing of the government's resources.

In comparison to the US, Argentina had less democratic institutions and a lower per capita income. My estimates indicate that the former led a stronger counter-cyclical policy response of financing, while the latter, made preventing its collapse comparatively more difficult. The main difference between Argentina and the US, however, was the structure of the country's fiscal system and the scope of fiscal reforms undertaken as a result of the Depression. The provisional government set itself the task of reducing the country's reliance on custom duties and indirect taxes by increasing direct taxation from an extremely low starting point. The first income tax in the country's history was announced in 1931 and introduced in 1932, amid considerable political resistance (Alhadeff, 1985) and the rebound in government financing after 1932 can be attributed largely to this new tax, as well as to a partial improvement in economic conditions. Nonetheless, income taxation continued to play a considerably smaller role in Argentina compared to the more advanced



(a) Dynamics of economic activity and fiscal aggregates



(b) Government financing counterfactuals

Figure 4.4: Argentina counterfactual, 1927-33

The NGDP, government financing and tax revenues are annual percentage changes. The fiscal balance as a share of GDP is in levels. Counterfactuals are based on the HT model for the TAX/NGDP fiscal capacity indicator for 1914-26

European and North American fiscal systems for the rest of the interwar period.

Even this relatively modest improvements in fiscal capacity, however, would have made a big difference in terms of fiscal policy had they taken place before the Depression hit. The gray line in the graph suggests that, had Argentina attained the level of capacity it had 1937 before the outbreak of the Depression, the government might have been able to obtain an essentially acyclical path in government financing instead of a markedly pro-cyclical one. The dark blue line, instead, indicates that obtaining the fiscal development

of the United Kingdom might have allowed the country to run a strongly counter-cyclical fiscal policy.

4.6 Exploring the channels: fiscal capacity and borrowing

The results of the analysis so far indicate that fiscal capacity affected the revenue smoothing ability of countries in the interwar period by influencing non-tax revenues. The most natural candidate for performing this function is borrowing because of both its large size compared to other non-tax revenues and its direct link to fiscal development and institutional quality.

Figure 4.5 illustrates the close relationship between fiscal capacity and borrowing in the interwar period. The charts illustrates the positive correlation between fiscal capacity (as measured by tax revenue over GDP) and the debt-to-GDP ratio at the central – panel (a) – and central and local – panel (b) – level for 1927-38. In the analysis below, I show that high capacity countries did not simply have a higher stock of debt throughout the interwar years, but were also able to accumulate more debt with respect to low capacity countries during this period. Moreover, borrowing was not only determined by the accumulated past fiscal capacity, but was facilitated by the reforms undertaken by some countries in the interwar years. I furthermore show that higher fiscal capacity countries faced lower borrowing costs. As a final step, I explore the two principal channels that could explain these findings. One is that a higher fiscal capacity simply signaled a higher present value of future tax revenues available for debt repayment. The other is that fiscal capacity signaled institutional quality and credibility making access to borrowing easier. The results support the latter channel.

4.6.1 Fiscal capacity and borrowing capacity

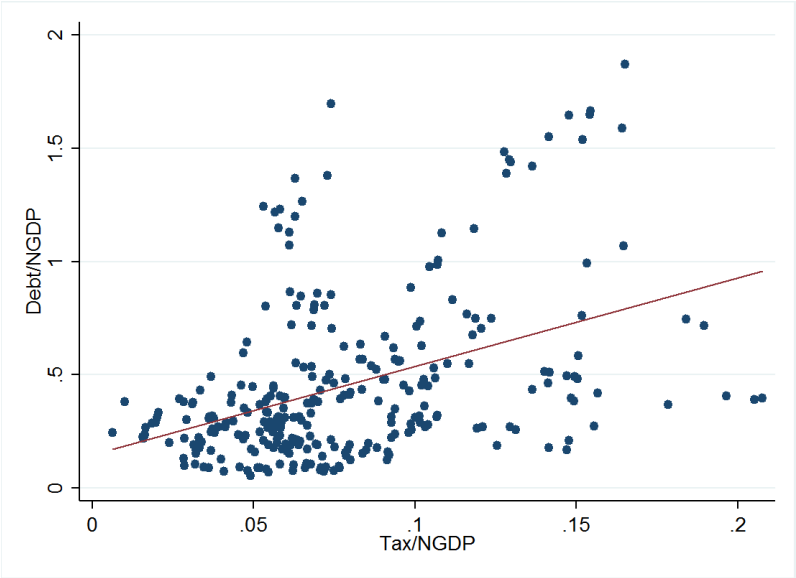
I relate fiscal capacity to debt levels though the following basic model:

$$lnDebt/NGDP_{i,t} = \beta_0 + \beta_1 FiscalCapacity_{i,t} + \mathbf{x_{i,t}}\gamma + \epsilon_{i,t} \tag{4.4}$$

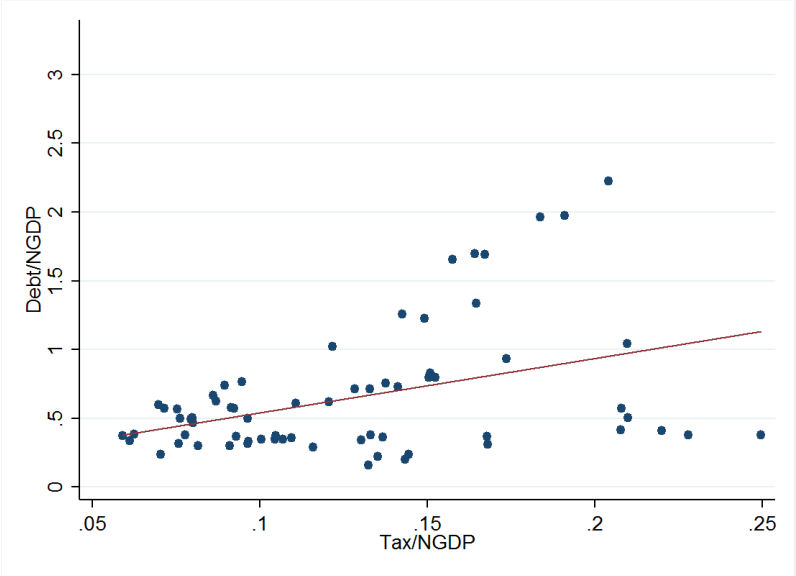
which takes the more general form:

$$lnDebt/NGDP_{i,t} = \beta_0 + \mathbf{A}(\ell)\{lnDebt/NGDP_i + FiscalCapacity_i + \mathbf{x_i}\} + c_i + l_t + \epsilon_{i,t} \tag{4.5}$$

when dynamics and fixed effects are introduced. $\mathbf{A}$ is a matrix of polynomials in the lag



(a) Central Government



(b) Central & Local Government

Figure 4.5: Fiscal Capacity and Government Debt, 1927-38

Source: for details on the tax revenue data see 4.B, for the debt data see 3.B; the nominal non-PPP adjusted GDP data is from Klasing and Milionis (2014)

operator, ℓ is an aribitrary number of lags, l and c are country and time fixed effects respectively, and ϵ is the idiosyncratic error term.

I begin the analysis by running static pooled estimations (Table 4.15). In columns 1-2 and 5-6 the natural logarithm of the average debt-to-GDP ratio for 1927-38 is regressed against average 1914-26 fiscal capacity indicators and controls, using OLS and LIML IV estimators respectively. The instruments for fiscal capacity are the same used in the analysis above. In columns 3-4 and 7-8 I repeat the exercise using annual values of the

debt-to-GDP ratios. The results indicate that, even after controlling for the structure of tax revenues, economic development (as measured by GDP per capita) and the level of democracy (as measured by the Polity2 score), higher fiscal capacity countries were able and/or willing to borrow more.

Table 4.15: Static estimation for debt-to-GDP ratios

					Panel A: Second Stage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	OLS	OLS	IV	IV	IV	IV
LN Debt/NGDP								
Tax/NGDP	14.07***		9.506***		19.25***		21.67**	
	(3.998)		(2.675)		(6.135)		(9.607)	
IncomeTax/NGDP		23.88**		29.17***		39.72***		44.06**
		(9.045)		(4.854)		(15.10)		(17.69)
Constant	-2.981***	-2.082***	-2.576***	-2.053***	-3.042***	-2.272***	-3.289***	-2.230***
	(0.752)	(0.643)	(0.602)	(0.591)	(0.572)	(0.490)	(0.768)	(0.540)
Additional controls								
Polity2	✓	✓	✓	✓	✓	✓	✓	✓
GDPPC	✓	✓	✓	✓	✓	✓	✓	✓
IncomeTaxShare	✓	✓	✓	✓	✓	✓	✓	✓
IndirectTaxShare	✓	✓	✓	✓	✓	✓	✓	✓
Observations	23	23	287	287	23	23	282	282
R-squared	0.457	0.405	0.302	0.335	0.521	0.485	0.073	0.262
Robust standard errors in parentheses								
*** p<0.01, ** p<0.05, * p<0.1								
Panel B: First Stage								
					Tax/NGDP	IncomeTax/NGDP	Tax/NGDP	IncomeTax/NGDP
YearsAtWar					0.00048***	0.000244**	0.0003559**	0.0002237**
					(0.0001264)	(0.0000921)	(0.0001576)	(0.0000888)
NatDisasterNumb					-0.0015283**	-0.0006687*	-0.002047***	-0.0007248***
					(0.0006689)	(0.00034)	(0.0005216)	(0.0001778)
F-Statistic					12.3279***	4.68507**	13.015***	9.34125***
Shea's Adj Part R^2					0.4211	0.5214	0.2921	0.4402
Robust standard errors in parentheses								
*** p<0.01, ** p<0.05, * p<0.1								

Time farme: 1927-38. Countries included in columns 1, 2, 5, 6 are: Argentina, Australia, Austria, Belgium, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, South Africa, Spain, Sweden, Switzerland, the United Kingdom and the United States; columns 3, 4, 7 and 8 additionally feature Brazil, Greece, Hungary, Ireland, Romania and Yugoslavia.

In Table 4.16, I add another important layer to my analysis by making the estimations dynamic (see the table notes for details on the different specifications in the four columns). This is important for four reasons. First, the debt-to-GDP ratio exhibits a high level of persistence; failing to account for this would lead to miss-specification and bias in the estimates. Second, by introducing lagged terms of the dependent and independent variables in the model, I allow these variables to interact with each other, reducing the possibility of omitted variable bias. Third, dynamic estimation using Generalized Method

of Moments (GMM) estimators allows me to instrument the regressors with more distant lags of themselves further reducing the possibility of bias. In the table, I present results obtained with the the difference GMM estimator (Arellano and Bond, 1991), but similar results also emerge with the system GMM estimator (Blundell and Bond, 1995). The latter estimator is more efficient, but the difference estimator is preferable in this context because the debt-to-GDP was very likely non-stationary in this period. Last, but not least, by using a dynamic framework, I am able to show that: 1) high capacity countries did not simply have a larger debt stock, but were able to borrow relatively more during the interwar years, 2) debt levels responded dynamically to changes in fiscal capacity taking place in the interwar years.

Table 4.16: Dynamic estimation for the debt-to-GDP ratios

	(1)	(2)	(3)	(4)
VARIABLES	LN Debt/NGDP	LN Debt/NGDP	LN Debt/NGDP	LN Debt/NGDP
L.LN Debt/NGDP	0.264	0.437***	0.772***	0.853*
	(0.172)	(0.138)	(0.110)	(0.483)
Tax/NGDP	11.61***	7.682***	7.801***	9.103**
	(2.925)	(1.718)	(2.547)	(4.262)
L.Tax/NGDP	-3.641	-2.250	-4.345**	-3.822
	(3.849)	(3.041)	(1.784)	(4.526)
Additional controls				
ΔNGDP	✓	✓	✓	✓
Polity2	✓	✓	✓	✓
IncomeTaxShare	✓	✓	✓	✓
IndirectTaxShare	✓	✓	✓	✓
Lag of controls	✓	✓	✓	✓
Country FE	✓	✓	✓	✓
Time FE				✓
Observations	192	192	200	200
Number of countries	31	31	31	31
Robust standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

L. stands for lagged. All changes (Δ) are percentage changes. I employ the two-step difference GMM estimator and insert one lag of the dependent variable as a regressor. I use the 4th lag of LN Debt/NGDP, ΔNGDP and Tax/NGDP as instrument for these variables for which I insert the contemporaneous and lagged values for all columns except column 4, where i use the 4th and 5th lag, in order to increase the explanatory power of the instruments, which is diminished by the use of time fixed-effects. The rest of the regressors are not instrumented in column 1 and instrumented with their 1st lag in the rest of the specifications. The results are robust to different lag structures and instrumenting. Standard error are robust in all specifications and small sample adjustments are employed. Columns 3 and 4 employ orthogonal differences proposed by Arellano and Bover (1995) rather than first differences in order to preserve sample size in the presence of gaps in the data. See Roodman (2009) for the details on the Stata command used in the estimations. Time frame: 1927-38. Countries included: Argentina, Australia, Austria, Belgium, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, South Africa, Spain, Sweden, Switzerland, the United Kingdom, the United States, Brazil, Egypt, Greece, Hungary, Ireland, Poland, Romania and Turkey.

The results are consistent across specifications and confirm the large and statistically significant effect of fiscal capacity found in the static estimations: high capacity countries were systematically able and/or willing to borrow more. This is true even if country fixed effects, which account for time-invarying country characteristics, time fixed effects, which account for common shocks, and the size of the lagged debt stock are controlled for.

In order to identify the channel through which fiscal capacity led to more borrowing, I proceed with two further steps. The first is to show that fiscal capacity had a positive effect only on long term borrowing and not on short term borrowing. This exercise is informative because long-term borrowing and institutional quality are tightly linked. Long term borrowing in the interwar period was the result of a planning and budgeting process, while short-term borrowing was often associated with emergency funds needed to cover shortfalls in revenues. Moreover, countries normally accessed long-term borrowing by floating bonds on capital markets, both international and domestic, and thus by submitting themselves to the scrutiny, however imperfect, of underwriters and lenders. The association between long term borrowing and creditworthiness is still relevant today: developing countries often choose to borrow short term because it tends to be cheaper, particularly during crises. This is due to the higher credit risk and uncertainty associated with long-term investments in counties with weak institutions and volatile macroeconomic fundamentals (Broner, Lorenzoni, and Schmukler, 2013).[53]

The second step is to test whether the source of tax revenues matters. As discussed in detail in Section 2.1, income taxation is strongly associated with fiscal capacity whereas a high reliance on trade taxes proxies a low level of fiscal development. Finding that not only the level, but also composition of revenues mattered would be a strong indication that the channel through which fiscal capacity influenced the ability to borrow in the interwar period was institutional quality, rather than simply a higher present value of future tax receipts.

Column 1 in Table 4.17 shows that a higher fiscal capacity is associated with more long term borrowing. For short-term borrowing, instead, there no statistically significant relationship with fiscal capacity (column 2). The results also demonstrate that a higher share of income taxes in GDP is associated with more borrowing, whereas there is no statistically significant relationship between trade taxes and borrowing. This is true for both the total debt-to-GDP ratio (column 3), and when only long term borrowing is

[53] Another explanation is that short-term borrowing serves as a commitment mechanisms, which disciplines countries into following sound policies due to avoid rollover crises, see references in Broner, Lorenzoni, and Schmukler (2013), page 63)

considered (column 4). When short term borrowing is considered, instead, the relationship is no longer statistically significant (column 5). The results thus lend support to the institutional quality channel.

Table 4.17: Debt-to-GDP ratios: long-term vs short-term borrowing and channels

	(1)	(2)	(3)	(4)	(5)
VARIABLES	LN LT Debt/NGDP	LN ST Debt/NGDP	LN Debt/NGDP	LN LT Debt/NGDP	LN ST Debt/NGDP
L.LN Debt/NGDP			0.797***		
			(0.0910)		
L.LN LT Debt/NGDP	0.778***			0.711***	
	(0.168)			(0.122)	
L.LN ST Debt/NGDP		0.598***			0.651***
		(0.171)			(0.143)
Tax/NGDP	9.195***	10.56			
	(1.919)	(6.242)			
L.Tax/NGDP	-5.982***	1.193			
	(2.079)	(6.834)			
IncomeTax/NGDP			19.87**	18.76**	30.17
			(9.483)	(6.862)	(31.63)
L.IncomeTax/NGDP			-10.72	-8.062	-17.48
			(6.895)	(7.771)	(34.14)
TradeTaxGDP			4.262	7.803	-23.63
			(9.848)	(9.811)	(29.84)
L.TradeTaxGDP			-1.044	-4.894	39.20
			(11.02)	(9.405)	(48.05)
		Additional controls			
ΔNGDP	✓	✓	✓	✓	✓
Polity2	✓	✓	✓	✓	✓
IncomeTaxShare	✓	✓	✓	✓	✓
IndirectTaxShare	✓	✓	✓	✓	✓
Lag of controls	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓
Observations	191	182	200	191	182
Number of countries	31	30	31	31	30
		Standard errors in parentheses			
		*** p<0.01, ** p<0.05, * p<0.1			

L. stands for lagged. All changes (Δ) are percentage changes. Except for the dependent variable, the specifications in this section are the same as those of column 3 of Table 4.16. Time frame: 1927-38. Countries included in columns 1,3 and 4 are: Argentina, Australia, Austria, Belgium, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, South Africa, Spain, Sweden, Switzerland, the United Kingdom, the United States, Brazil, Egypt, Greece, Hungary, Ireland, Poland, Romania and Turkey. Columns 2 and 5 exclude Egypt due to data availability.

4.6.2 Fiscal capacity and borrowing costs

To complete the analysis of the effect of fiscal capacity on borrowing, I turn to analyzing borrowing costs. The outcome variable is the spread of domestic bond yields over the US domestic bond yield (see the Appendix 4.B for details on this data). The underlying assumption is that yields reflect the expected probability of default, and that yield spreads

reflect the expected probability of default relative to a baseline bond or country of issue with very low or zero default risk (Tomz and Wright, 2013).[54] I choose the US yield as the baseline because the country's economic size and high level of financial development contributed to making it my samples' lowest domestic yield for the entire period under consideration (Figure 4.6).[55]

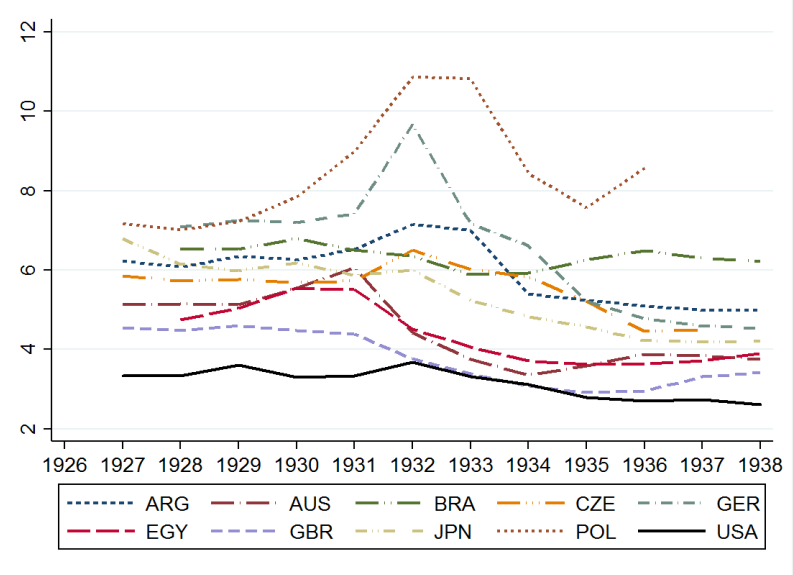


Figure 4.6: Domestic bond yields in selected countries, 1927-38

Yields expressed in percentage points. ARG=Argentina, AUS=Autralia, BRA=Brazil, CZE= Czechoslovakia, GER=Germany, EGY=Egypt, GBR= United Kingdom, JPN=Japan, POL=Poland, USA=United States of America. Source: League of Nations (1936/37, 1937/38, 1939/40); see Appendix 4.B for details.

I focus on domestic financial markets, for two reasons. The first is that the availability of data on domestic bond yields is greater. More important, however, is the fact that with the onset of the Great Depression, which all but froze international financial markets, domestic credit became the main source of government financing: the domestic debt stock of central governments in my sample increased from around 50% on average in 1927-29 to around 60% in 1933-38.[56]

The other fundamental hypothesis of the exercise is that the strength and credibility

[54] Tomz and Wright (2013) also point out some pitfalls of working with bond yields. First, the assumption is that borrowing takes place in the form of the emission of bonds on competitive markets. this however is not always the case. Alternative sources of borrowing are banks, although this was uncommon in the interwar period, but also international organizations and central banks which do not necessarily lend at market rates. Second, not all sovereign bonds are actively traded on liquid markets.Third, contractual features can vary across countries and different bond issues, impacting yields.

[55] See Basile, Landon-Lane, and Rockoff (2010) for a thorough discussion of interest rates in the US in the interwar period.

[56] Countries included in this calculation are: Austria, Belgium, Bulgaria, Czechoslovakia, Denmark, Finland, Germany, UK, Greece, Hungary, Ireland, Italy, Norway, Poland, Romania, Sweden, Switzerland, Argentina, Bolivia, Brazil, Chile, Colombia, Peru, Uruguay, Venezuela, Australia, Japan, Canada, Egypt, New Zealand, US, South Africa, India and Spain.

of fiscal systems cannot be captured by introducing short-term policy variables, such as budget deficits, as is commonly done in the literature. I argue, instead, that my fiscal capacity indicators capture the deeply-rooted and structural characteristic of countries that determine their fiscal development and broader institutional quality, and are thus the main variables of interest.

The estimating equation is as follows:

$$lnBondYieldSpread_{i,t} = \beta_0 + \beta_1 lnFiscalCapacity_{i,t} + \mathbf{x_{i,t}}\gamma + c_i + l_t + \epsilon_{i,t} \tag{4.6}$$

where $\mathbf{x}$ is a vector of controls, c and l are country and time fixed effects respectively and $\epsilon_{i,t}$ is the idiosyncratic error term.

In the regressions, I distinguish between countries on and off the Gold Standard in order to account for the possibility of Gold Standard membership acting as "good housekeeping seal of approval". The role of Gold Standard adherence on borrowing costs has been much debated in the literature. Bordo and Kydland (1995); Bordo and Rockoff (1996) argue that, indeed, during the heyday of the classical Gold Standard (1870-1913) long-standing adherence to this international monetary system signaled the pursuit of orthodox policies, which significantly lowered borrowing costs. Bordo, Edelstein, and Rockoff (1999) find a similar effect for countries returning to gold in the 1920s, particularly if this was done at the pre-WWI parity, which, the authors argue, was a strong signal of financial rectitude. Obstfeld and Taylor (2003) confirm these results of the pre-WWI era, but not for the 1920s, for which they record the rising importance of debt burdens and British Empire membership.[57] Flandreau and Zumer (2004) and Alquist and Chabot (2011), instead, find that sound policies and common risk factors were more important than Gold Standard adherence even before WWI. In any case, in my context accounting for Gold Standard membership also serves the purpose of controlling for the progressive disintegration of international financial markets over the course of the 1930s.

Table 4.18 presents the results of the analysis. Column 1-3 illustrate the pooled OLS, columns 4-6 the FE, columns 7-9 the FD estimates. Columns 1, 4 and 7 present a simple model in which the bond yield spread is regressed against the Tax/NGDP fiscal capacity

[57] The effect of British Empire membership is another controversial topic. Ferguson and Schlularick (2006), for example, find that colonies enjoyed lower borrowing costs than non-colonies while Accominotti, Flandreau, and Rezzik (2011) argue that colonies were structurally different from non-colonies due to the implicit monitoring by the British government. The authors thus refute the idea of Empire being a "marginal" effect to be identified though a dummy *ceteris paribus*.

Table 4.18: Determinants of domestic government bond yield spreads vis-a-vis the US

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	OLS	OLS	OLS	FE	FE	FE	FD	FD	FD	HT	HT
LN Spread											
Panel A: Pooled On & Not On Gold											
LN Tax/NGDP	0.406**	0.134		-0.317**	-0.230**		-0.254**	-0.236**		-0.865***	
	(0.194)	(0.198)		(0.114)	(0.110)		(0.104)	(0.113)		(0.292)	
Panel B: On Gold											
LN Tax/NGDP			0.102			0.0467			-0.127		-0.422
			(0.265)			(0.197)			(0.227)		(0.535)
Panel C: Not On Gold											
LN Tax/NGDP			0.117			-0.225**			-0.262*		-0.853**
			(0.228)			(0.107)			(0.151)		(0.337)
Constant	0.896*	7.507***	7.128***	0.364	15.18**	14.39**	0.107	0.183*	0.190**	11.96***	10.34***
	(0.500)	(1.693)	(1.758)	(0.348)	(6.183)	(6.805)	(0.109)	(0.0899)	(0.0854)	(4.082)	(3.350)
											10.37**
											(4.327)
Additional controls											
Default size	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP		✓	✓		✓	✓		✓	✓	✓	✓
LN Debt/NGDP		✓	✓		✓	✓		✓	✓	✓	✓
LN Openness		✓	✓		✓	✓		✓	✓	✓	✓
LN GDPPC		✓	✓		✓	✓		✓	✓	✓	✓
Polity2		✓	✓		✓	✓		✓	✓	✓	✓
On Gold		✓			✓			✓		✓	
Country FE				✓	✓	✓	✓	✓	✓	✓	✓
Time FE				✓	✓	✓	✓	✓	✓	✓	✓
Observations	221	209	209	221	209	209	189	180	180	206	72-134
Number of countries				27	25	25	27	25	25	20	19
R-squared	0.378	0.553	0.577	0.227	0.283	0.339	0.212	0.216	0.274		
Robust standard errors in parentheses											
*** p<0.01, ** p<0.05, * p<0.1											

All changes (Δ) are percentage changes. Time frame: 1927-38. Countries included in columns 1, 4 and 7 are: Argentina, Australia, Austria, Belgium, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, Spain, Sweden, Switzerland, the United Kingdom, Brazil, Egypt, Greece, Hungary, India, Poland and Romania; columns 2, 3, 5, 6, 8 and 9 exclude Egypt due to data availability. Column 10 features: Argentina, Australia, Austria, Belgium, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway Spain, Sweden, Switzerland, and the UK; column 11 excludes Spain as it was never on gold in this period.

indicator, the size of the default as computed in Paper 1 (Chapter 3),[58] and the debt-to-GDP ratio also from Paper 1. In the columns 2,5, and 8, the model is enriched with further controls: the growth rate of nominal GDP calculated from data provided by Klasing and Milionis (2014), openness as measured by the share of trade in GDP provided by the same authors, GDP per capita from Bolt and van Zanden (2013) and an on gold dummy based on gold adherence dates summarized by Crafts and Fearon (2013).[59] In columns 3, 6 and 9, I run separate estimations for countries on gold and off gold to account for structural rather than marginal differences between countries adhering or not to the international monetary arrangement. In columns 10 and 11, I re-run the model using the HT estimator in order to employ the 1914-26 average of the controls rather than yearly values. This reduces the possibility of reverse causality between bond yield spreads and the right hand side variables.

Besides reducing the risk of omitted variable bias, the inclusion of fixed effects, also minimizes issues of cross-country comparability of the data. This is important because the bonds of different countries featured different contractual characteristics. Studying the evolution of domestic bond yields through time offers a far better indicator of default probabilities than raw comparisons across countries. By comparing the OLS and panel estimates, it is immediately apparent that failing to account for fixed effects is a very serious source of bias. In the OLS estimates, a higher debt-to-GDP ratio is associated with lower borrowing costs (not shown), while a higher fiscal capacity is associated with higher borrowing costs, even though once more controls are included the result becomes insignificant. Once fixed effects are included, it emerges clearly that a higher debt-to-GDP ratio increases borrowing costs, while more fiscal capacity decreases them, in line with my hypothesis. A further intuitive result that emerges from the estimates, although it is not significant in all specifications, is that countries with a higher GDP per capita had lower borrowing costs.

The estimation also clearly indicates that controlling for Gold Standard adherence with a simple dummy is inadequate. This echoes Accominotti, Flandreau, and Rezzik (2011)'s argument for British Empire membership before WWI that differences between empire and non-empire countries were structural and cannot be captured by just allowing

[58] This is the share of the principal of dollar denominated bonds in default over the total principal dollar-denominated bonds. The measure is developed in the paper as a measure of default size on impact, which purposely does not take into account ex-post elements such as the final haircut imposed on creditors, which in most cases of defaults in the interwar period was only finalized after long negotiations, some of which wee settled only after the end of WWII.

[59] I consider countries to be off gold if they have officially left the Standard or have introduced exchange controls.

for a different intercept. Indeed, the coefficient of the Gold Standard dummy cannot be statistically distinguished from zero in any specification, which suggests that Gold Standard membership was not a direct constraint on borrowing. However, once separate estimations are run for countries on and off gold, a different pattern emerges. Specifically, I find that fiscal capacity reduced borrowing costs for countries off gold, but not for countries on gold.

This results represents a strong suggestion that, for countries not on gold, fundamentals such fiscal capacity mattered more than for countries not on gold. One can hypothesize that, without Gold Standard adherence to guide them, the strength of the fiscal system became more important as a guiding principle for investors. The fact that, as the interwar debt crisis unfolded and the Gold Standard disintegrated, rating agency Moody's increased the amount of data it supplied in its manuals, with tax revenues being a big part of this, supports this hypothesis. Thus, while fiscally stronger countries might have benefitted from an easier access to borrowing once free from the *golden fetters*, weaker ones might have actually experienced an increase in their bond yield spreads. This hypothesis provides an important qualification to much of the literature, which argues that Gold Standard membership was the only factor that constrained countries' policy responses to the Depression (Temin, 1989; Eichengreen, 1992).

This result also speaks to findings by Bernanke and James (1991) and Bernanke (1995) that economic conditions in countries that left the Gold Standard in 1931 and those that did not were quite similar, while the recovery was much more robust in leavers thereafter. The authors, in line with Eichengreen (1992), attribute this to the policy freedom that leaving the fixed exchange rate provided, although they stress monetary policy, whereas Eichengreen also outlined an important role for fiscal policy. The authors further dismiss endogeneity concerns regarding the decision to abandon gold, highlighting that weaker countries should have left the standard earlier and would have recovered more strongly than those which actually left. This reasoning, however, rests on the assumption that macroeconomic fundamentals can be captured by looking at short-term indicators and that leaving the Gold Standard would have had similar effects in all countries, no matter their institutional characteristics. My findings indicate that, on the contrary, differences in deeply-rooted fundamentals like fiscal capacity might have led to asymmetric effects of the decision to abandon the Gold Standard casting doubts on the idea that an early departure from Gold would have led to less contractionary policies and a faster recovery in all countries

The quantitative impact of fiscal capacity is fairly large. The estimates indicate that a 1% increase in fiscal capacity led to a lower bound decrease in the yield spread vis-a-vis the US of around 0.23%. This means that a 1 percentage point increase in fiscal capacity – which on average represented a 12.5% increase in the share of taxes in GDP – would have decreased of the spread vis-a-vis the US by around 8 basis points. The estimated difference in the spread between the country with the highest fiscal capacity and the average country would have been over 100 basis points. When I employ the average 1914-26 fiscal capacity indicator and the HT estimator, the effect is about 4 times stronger: a 1% increase in the share of taxes in GDP lowered yield spreads by around 0.87%.

Table 4.19: Government bond yield spreads: institutional quality vs higher revenues

	(1)	(2)	(3)
	HT	HT	HT
VARIABLES	LN Spread	LN Spread	LN Spread
		On Gold	Not on Gold
LN Tax/NGDP	-0.173	0.176	0.132
	(0.227)	(0.390)	(0.238)
LN IncomeTax/NGDP	-0.367*	-0.0971	-0.318*
	(0.213)	(0.264)	(0.191)
LN TradeTax/NGDP	0.350	0.448*	0.0692
	(0.302)	(0.235)	(0.290)
Constant	11.73***	11.25***	6.944*
	(4.111)	(1.960)	(3.665)
Additional controls			
ΔNGDP	✓	✓	✓
LN Debt/NGDP	✓	✓	✓
DefaultSize	✓	✓	✓
LN Openness	✓	✓	✓
LN GDPPC	✓	✓	✓
Polity2	✓	✓	✓
Country FE	✓	✓	✓
Observations	172	70	102
Number of countries	20	19	19
Robust standard errors in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

All changes (Δ) are percentage changes. Time frame: 1927-38. All variables are 1914-26 values except for ΔNGDP, LN Debt/NGDP, DefaultSize and LN Tax/NGDP, which are yearly values. Countries included in column 1 are: Argentina, Australia, Austria, Belgium, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway Spain, Sweden, Switzerland, and the UK. Columns 2 and 3 exclude Spain, which was never on gold in this period.

As a final exercise, I provide some evidence of the channels through which fiscal capacity affected borrowing costs (Table 4.19). The hypothesis is that it not only mattered how much revenue countries raised, but also how. I run the same model as above using the HT estimator to minimize reverse causality concerns and I insert the 1914-26 average shares of trade and income taxes over GDP, while controlling for the yearly tax revenue as a share

of GDP. Once again, I distinguish between off and on gold countries. The results indicate that, indeed, how revenue was raised mattered a great deal, particularly in the absence of the Gold anchor. A 1% higher share of income taxes in GDP lowered the yield spread vis-a-vis the US by almost 0.37% on average and over 0.3% for countries off gold. I find no significant effect for countries on gold considered in isolation and, more importantly, for trade taxes. Therefore, revenue per se did not reduce the perceived probability of default, it was rather the demonstration of being able to raise taxes in complex and demanding way that, by signaling institutional quality, allowed countries to finance their deficits more cheaply in the interwar period.

4.7 Conclusion

This paper investigated the impact of fiscal capacity on the dynamics of public revenues in the interwar period. To the best of my knowledge, this is a first. My findings show that a higher degree of fiscal capacity led to a lower volatility of government financing in response to economic shocks. The analysis further indicates that the channel through which fiscal capacity smoothed financing was by providing an easier access to borrowing for governments, whereas there is no conclusive evidence of fiscal capacity lowering the volatility of tax revenues. Finally, the results reveal that how tax revenue was raised mattered. This suggests that fiscal capacity allowed countries to borrow more extensively and more cheaply by signaling higher institutional quality, rather than simply a higher present value of future tax receipts.

The findings of this paper speak to four main strands of literature. First, they suggest that the literature on the Great Depression should take the deep-rooted constraints under which countries' governments operated more seriously into account in evaluating policy responses to the the slump. Gold Standard membership probably did impose a straitjacket on countries, which constrained their policy choices, but so did historically-determined factors, such as fiscal development. The two constraints probably interacted, leading to asymmetric effects of membership to the international monetary system: whereas shedding the golden fetters opened the possibility of a less restrictive fiscal policy for fiscally strong countries, my findings suggest that it might have been the opposite for fiscally weak ones. Second, the paper illustrates a new channel through which fiscal capacity can affect economic outcomes besides fostering long-term economic development. I show that fiscal capacity affected cyclical fiscal outcomes in interwar period, with deep potential repercussions on both the short and long-term economic and political health of countries. In

countries where financing volatility led to fiscal crises, which affected the action of states or led to regime changes, long-term consequences might have been particularly severe. Third, the findings of the paper lend support to the hypothesis that institutionally determined borrowing constraints can contribute to fiscal policy pro-cyclicality, and that the size of governments can affect their ability to be a stabilizing force in the economy.

Appendices

4.A Additional analysis and robustness checks

In this Appendix, I outline some further results which integrate the analysis in the main body of the paper.

4.A.1 Revenue elasticity: sensitivity to a changing the sample and including local governments

Table 4.20 and 4.21 demonstrate that the relationships between economic activity and tax revenues and economic activity and government financing are very similar if I consider a longer time period – 1920-1938 instead of 1927-38, which is the period of analysis in the paper – or include all available observations rater than restricting the sample to observations for which both outcome variables are available. While economic activity and tax revenues once again co-move almost 1-to-1, the coefficient of the nominal GDP when government financing is the outcome variable is less than half size. In column 6 of both tables, the coefficient even becomes statistically insignificant at conventional levels. This suggests that non-tax revenues played an important smoothing role in overall public revenue and that this was similar across the whole interwar period, notwithstanding the impact of the Great Depression and the turmoil financial markets underwent starting from 1929.

It is harder to draw inference when I include local governments due to the small size of the sample (Table 4.22). However, it wold appear that the 1) total government revenues were less sensitive to changes in output than central government revenues, 2) the smoothing effect of non-tax revenues is concentrated at the central level. The former is demonstrated by the smaller coefficient relating changes in nominal GDP to both tax revenues and government financing compared to that of central revenues; the latter is evident in the very similar coefficient I find for both tax revenues and government financing.

4.A.2 Fiscal capacity and revenue volatility: pooled IV results

I report the results of the pooled IV results for completeness and to further illustrate the relevance of my instruments. Table 4.23 presents the estimates. Panel A features the results obtained with the Limited Information Maximum Likelihood (LIML) estimator, whereas Panel B illustrates the first stage of the regressions. The LIML estimator is preferred to Two Stage Least Squares (2SLS) due to its greater robustness in the presence

Table 4.20: The elasticity of tax revenues and government nancing to changes in output, 1920-38

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	FE	FE	FD	FD
VARIABLES	ΔTaxRevenue	ΔGovtFinancing	ΔTaxRevenue	ΔGovtFinancing	ΔTaxRevenue	ΔGovtFinancing
ΔNGDP	1.037***	0.424***	1.124***	0.431***	1.084***	0.208
	(0.215)	(0.120)	(0.281)	(0.129)	(0.358)	(0.132)
Constant	0.0483**	0.0332***	0.0473***	0.0331***	-0.00329	0.00158
	(0.0212)	(0.00830)	(0.00294)	(0.00129)	(0.00701)	(0.00428)
Country FE			✓	✓	✓	✓
Observations	293	452	293	452	240	407
Number of countries	33	35	33	35	33	35
R-squared	0.052	0.047	0.065	0.051	0.027	0.009
Robust standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

All changes (Δ) are percentage changes. Countries included in columns 1, 3 and 5 are: Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Egypt, Finland, France, Germany, Greece, Hungary, India, Ireland, Italy, Japan, the Netherlands, Norway, Poland, Romania, South Africa, Spain, Sweden, Switzerland, Turkey, the United Kingdom, the United States and Yugoslavia; columns 2, 4 and 6 additionally feature Indonesia, New Zealand, and Uruguay, but exclude Egypt due to data availability.

Table 4.21: The elasticity of tax revenues and government nancing to changes in output, all observations 1927-38

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	FE	FE	FD	FD
VARIABLES	ΔTaxRevenue	ΔGovtFinancing	ΔTaxRevenue	ΔGovtFinancing	ΔTaxRevenue	ΔGovtFinancing
ΔNGDP	0.910***	0.519***	0.987***	0.506***	0.794**	0.228
	(0.186)	(0.111)	(0.272)	(0.111)	(0.333)	(0.148)
Constant	0.0448*	0.0261***	0.0442***	0.0262***	-0.00411	0.00205
	(0.0225)	(0.00519)	(0.00183)	(0.000281)	(0.00676)	(0.00396)
Country FE			✓	✓	✓	✓
Observations	278	325	278	325	239	307
Number of countries	33	35	33	35	33	35
R-squared	0.036	0.097	0.047	0.092	0.012	0.013
Robust standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

All changes (Δ) are percentage changes. Countries included in columns 1, 3 and 5 are: Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Egypt, Finland, France, Germany, Greece, Hungary, India, Ireland, Italy, Japan, the Netherlands, Norway, Poland, Romania, South Africa, Spain, Sweden, Switzerland, Turkey, the United Kingdom, the United States and Yugoslavia; columns 2, 4 and 6 additionally feature Indonesia, New Zealand, and Uruguay, but exclude Egypt due to data availability.

of weak instruments and the inclusion of many instruments. For the LIML estimator, critical values of the first stage F-statistic actually decrease with the number of instruments included, contrary to 2SLS (Stock, Wright, and Yogo, 2002; Stock and Yogo, 2005). The critical value for minimizing bias due to weak instruments with 4 instruments and

Table 4.22: The elasticity of central plus local tax revenues and government financing to changes in output,1927-38

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ΔTotTaxRev	ΔTotGovFin	ΔTotTaxRev	ΔTotGovtFin	ΔTotTaxRev	ΔTotGovtFin
ΔNGDP	0.344***	0.468**	0.331**	0.330	0.249	0.402**
	(0.0917)	(0.169)	(0.114)	(0.191)	(0.142)	(0.146)
Constant	0.00395	0.0233*	0.00349	0.0184**	-0.0136	-0.0144
	(0.00399)	(0.0115)	(0.00401)	(0.00676)	(0.00797)	(0.0137)
Country FE			✓	✓	✓	✓
Observations	46	46	46	46	35	35
Number of countries	11	11	11	11	11	11
R-squared	0.139	0.124	0.109	0.060	0.033	0.053
Robust standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

All changes (Δ) are percentage changes. Countries included are: Austria, Belgium, Denmark, France, Germany, Hungary, the Netherlands, Sweden, Switzerland, the UK and the USA.

2 regressors is 7.03 (see table 5.4 in Stock and Yogo (2005)), and is exceeded in all instances except one (for the interaction term in column 2). Robustness checks carried out with the Continuously Updated Generalized Method of Moments estimator (CUE), which is consistent with non-normal errors in the presence of many instruments (note that 4 instruments does not qualify as many instruments), yielded practically identical results, the only difference being larger standard errors as expected due to the estimator's wider dispersion (Hausman, Lewis, Menzel, and Newey, 2011).[60]

The table compares the estimates for tax revenues and government financing directly: columns 1, 2, 5 and 6 feature estimates for the former and columns 3, 4 ,7 and 8 for the latter. The first 4 columns present the baseline specification, whereas columns 5 to 8 control for the composition of tax revenues. The fiscal capacity coefficient is no longer significant in the specification with tax revenues as the dependent variable, which highlights the usefulness of the IV approach compared to OLS. This result is not due to large differences in the magnitudes of the coefficients compared to the OLS estimates, but to the standard errors being much larger. IV estimates are known to yield larger standard errors than their OLS counterparts, but the satisfactory first stage statistics and large increase in the errors are good reasons to believe that this might be due to the effect being in fact indistinguishable from zero. The panel results in the body of the paper further support this notion.

For changes in government financing, instead, the smoothing role of fiscal capacity

[60] As above, I use 1914-38 averages for the fiscal capacity indicators.

Table 4.23: Pooled instrumental variable estimation

				Panel A: Second Stage				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	ΔTaxRevenue	ΔTaxRevenue	ΔGovtFinancing	ΔGovtFinancing	ΔTaxRevenue	ΔTaxRevenue	ΔGovtFinancing	ΔGovtFinancing
ΔNGDP	1.123*	0.706*	2.161***	1.135***	1.639***	1.297***	2.145***	1.287**
	(0.591)	(0.366)	(0.691)	(0.368)	(0.624)	(0.428)	(0.730)	(0.529)
Tax/NGDP	-0.167		-0.240		-0.124		-0.212	
	(0.141)		(0.191)		(0.173)		(0.174)	
ΔNGDP*Tax/NGDP	-5.178		-13.73**		-6.092		-14.98**	
	(5.548)		(5.867)		(5.400)		(5.999)	
IncomeTax/NGDP		-0.393		-0.500*		-0.243		-0.459
		(0.309)		(0.303)		(0.363)		(0.301)
ΔNGDP*IncomeTax/NGDP		-3.728		-18.32**		-11.70		-30.86**
		(11.04)		(8.661)		(10.45)		(14.50)
Constant	0.0402**	0.0370***	0.0328	0.0262**	0.0153	0.0130	0.0164	0.00896
	(0.0178)	(0.0126)	(0.0244)	(0.0123)	(0.0179)	(0.0135)	(0.0204)	(0.0209)
				Additional controls				
Polity2	✓	✓	✓	✓	✓	✓	✓	✓
GDPPC	✓	✓	✓	✓	✓	✓	✓	✓
IncomeTaxShare	✓	✓	✓	✓	✓	✓	✓	✓
IndirectTaxShare	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*Polity2	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*GDPPC	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*IncomeTaxShare	✓	✓	✓	✓	✓	✓	✓	✓
ΔNGDP*IndirectTaxShare	✓	✓	✓	✓	✓	✓	✓	✓
Observations	239	239	266	266	208	208	219	219
R-squared	0.281	0.273	0.161	0.145	0.311	0.313	0.149	0.135
				Panel B: First Stage				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Tax/NGDP	IncomeTax/NGDP	Tax/NGDP	IncomeTax/NGDP	Tax/NGDP	IncomeTax/NGDP	Tax/NGDP	IncomeTax/NGDP
YearsAtWar	0.000555***	0.000368***	0.000506***	0.000368***	0.000498***	0.000262***	0.000489***	0.000264***
	(0.000127)	(0.000114)	(0.000156)	(0.000116)	(0.000096)	(0.000079)	(0.000116)	(0.00008)
NatDisastNumb	-0.00103**	0.0000402	-0.00112**	0.0000344	-0.00168***	-0.000713**	-0.00162***	-0.000703**
	(0.000465)	(0.000186)	(0.000523)	(0.000195)	(0.000580)	(0.000307)	(0.000576)	(0.000292)
ΔNGDP*YearsAtWar	0.000558***	0.000349***	0.000548***	0.000350***	0.000481***	0.000244***	0.000513***	0.000251***
	(0.000112)	(0.000110)	(0.000108)	(0.000103)	(0.000100)	(0.0000818)	(0.000111)	(0.0000835)
ΔNGDP*NatDisastNumb	-0.000980***	0.0000572	-0.001050***	0.000019	-0.001586**	-0.000657**	-0.001329**	-0.000601**
	(0.000367)	(0.000203)	(0.000378)	(0.000209)	(0.000635)	(0.000299)	(0.000572)	(0.000282)
F-Statistic	8.792***	17.855***	7.353***	11.790***	16.923***	7.036***	13.526***	11.828***
F-Statistic Interaction	8.5453***	6.4871***	13.11***	12.500***	10.224***	5.0642**	15.312***	15.156**
Shea's Adj Part R^2	0.306	0.469	0.2051	0.424	0.631	0.642	0.557	0.641
Shea's Adj Part R^2 Interaction	0.419	0.470	0.361	0.479	0.621	0.626	0.548	0.624
				Robust standard errors in parentheses				
				*** p<0.01, ** p<0.05, * p<0.1				

Time frame: 1927-38. I employ the LIML estimator. All changes (Δ) are percentage changes. Time frame: 1927-38. Countries included in columns 1-4 are: Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, the Netherlands, Norway, Romania, South Africa, Spain, Sweden, Switzerland, the United Kingdom, the United States and Yugoslavia; columns 5-8 exclude Brazil, Greece, Hungary, Ireland, Romania and Yugoslavia due to data availability,

is confirmed. The coefficients for the Tax/NGDP indicator are quantitatively almost identical to the OLS ones, whereas the IncomeTax/NGDP ones are significantly larger. The findings, also further corroborate the notion that the effects of fiscal capacity on revenues are strongly non-linear and that, failing to account for this, would lead to serious misspecification issues.

4.A.3 Instrumenting changes in output

As an additional robustness check, I instrument changes in output using changes in output in trading partners weighted by pre-Depression (1926) bilateral trade flows (see Appendix

4.B for sources). I not only instrument the fiscal capacity indicators and the changes in output, but also all the other interaction variables using the weighted partners output changes interacted with the regressors. Table 4.24 outlines the result. Not all instruments are very strong, as demonstrated by the large standard errors. I use the Fuller-k estimator to partially account for this. In any case, the results are clear cut and consistent with the rest of the paper when I use the FE estimator. With the FD estimator, the coefficients are even larger, but the reduction in the variability of the data due to first differencing further increases the standard errors rendering the coefficient statistically insignificant. This, however, in no way affects the conclusions of the paper that fiscal capacity had a strong smoothing effect for government financing in the interwar period.

Table 4.24: Panel IV estimation using the weighted GDP of trading partners as instrument

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	FE	FD	FD	FD	FD
VARIABLES	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtFinancing	ΔGovtExpenditure	ΔGovtExpenditure
ΔNGDP	3.678**	2.629*	4.555*	3.206	2.928***	2.305***
	(1.818)	(1.465)	(2.721)	(2.053)	(0.950)	(0.782)
ΔNGDP*Tax/NGDP	-23.32*		-28.83		-13.44*	
	(12.33)		(19.64)		(6.941)	
ΔNGDP*IncomeTax/NGDP		-47.91*		-53.58		-27.06**
		(28.57)		(40.06)		(11.91)
Constant			0.0129*	0.0125	-0.00490	-0.00538
			(0.00766)	(0.00808)	(0.00793)	(0.00779)
			Additional controls			
ΔNGDP*Polity2	✓	✓	✓	✓	✓	✓
ΔNGDP*GDPPC	✓	✓	✓	✓	✓	✓
ΔNGDP*IncomeTaxShare	✓	✓	✓	✓	✓	✓
ΔNGDP*IndirectTaxShare	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Observations	219	219	191	191	182	182
Number of countries	23	23	23	23	23	23
R-squared	0.154	0.120	-0.170	-0.189	0.063	0.063
			Robust standard errors in parentheses			
			*** p<0.01, ** p<0.05, * p<0.1			

Fuller-k estimation. All changes (Δ) are percentage changes. Time frame: 1927-38. Countries included: Argentina, Australia, Austria, Belgium, Bulgaria, Canada, Chile, Colombia, Czechoslovakia, Denmark, Finland, France, Germany, Italy, Japan, the Netherlands, Norway, South Africa, Spain, Sweden, Switzerland, the United Kingdom and the United States.

4.A.4 Further robustness checks

I have carried out a broad range of further robustness checks, which I do not show due to the already considerable length of this paper. The main result of the paper – the smoothing effect of fiscal capacity for government financing – is robust to using an extended sample (1920-38) and to using 5 year moving avarages for fiscal capacity indicators rather than 1914-26 averages. This is true for pooled OLS, Panel, IV and Panel IV estimations. As

mentioned in the text of the paper, the inclusion of further controls – such as gold standard membership, trade openness, the size and composition of the public debt burden, and the terms of trade – also do not affect the result.

4.B Data description and sources

Nominal non-PPP-adjusted GDP (NGDP) and Openness: I use the nominal, non-PPP-adjusted GDP figures estimated by Klasing and Milionis (2014) for the period 1870-1949. These are based on Maddison's GDP estimates and obtained using the so-called "short-cut method". This method has a long history; Prados de la Escosura (2000) offers a detailed description. In essence, it exploits the relationship between PPP adjusted and non-PPP-adjusted GDP determined by the relative prices of traded and non-traded goods and the relative income level of the country compared to the benchmark country. In doing this, it makes use of the Balassa-Samuelson theorem. These data offer clear advantages compared to standard GDP figures in constructing measures of the debt burden, given that the latter are also in unadjusted nominal terms. They also capture an important additional feature of the Great Depression besides the contraction of output: the huge deflation that accompanied it.

Trade openness: Openness figures are from Klasing and Milionis (2014). The authors obtain the openness figures by combining their estimates of nominal GDP with trade data from Barbieri, Keshk, and Pollins (2009).

Bilateral trade: I use bilateral trade data from Barbieri, Keshk, and Pollins (2009) and Barbieri and Keshk (2016) in 1926 to calculate the Pre-Depression weights to assign to changes in output of the trading partners with which I instrument changes on output.

GDP per capita (GDPPC): GDP per capita is taken from the Maddison's Project's latest update (Bolt and van Zanden, 2013), which incorporates the latest available estimates.

Polity2: This is the polity2 score from the POLITY IV database (Marshall and Jaggers, 2005). It is a combined score of autocracy and democracy (both measure between 0 and 10) and is obtained by subtracting the autocracy score from the democracy one. The two scores are weighed indicators of the competitiveness of political participation, the openness and competitiveness of executive recruitment and constraints on the chief executive.

Central & local debts and debt service: See Appendix 3.B.

Years at War: these are the years spent fighting major external conflicts by a country in the period 1816-1913 as reconstructed by Dincecco and Prado (2012) based on Clodfelter

(2002)'s database of major external conflicts in Europe, Africa, the Middle East, Asia and Oceania between 1500 and 2000. The variable takes into account the fact that a country might be involved in more than one conflict per year and can thus exceed 97, as in the case of the United Kingdom.

Natural Disasters: is from the EM-DAT database of the Centre for the Epidemiology of Disasters based at the Université Catholique de Louvain (CRED, 2017), which is the most commonly used sources for studies on natural disasters. The data refers to all natural disasters to hit countries in the sample between 1900 – the starting state of the database – and 1926 – the starting date of the analysis. The CRED definition of natural disasters implies one of these conditions: 1) ≥ 10 casualties, 2) ≥ 100 people affected, 3) declaration of a state of emergency, 4) call for international assistance. I have enquired about the reliability of these data for the early 20th century directly with CRED. As discussed in the body of the paper, they believe that the data underestimates the number of disasters, particularly small disasters, in poorer countries. This indicates that a more accurate reporting would strengthen my results.

Domestic Bond Yields: These data are from various publications of the are of the League of Nations: League of Nations (1939/40) pages 251-54, Table 117; League of Nations (1937/38), pages 256-60, Table 131; League of Nations (1936/37), pages 245-59, Table 127. Bond type (coupon %): Australia Government bonds (4); Austria Government Bonds (miscellaneous) Argentina Government (5); Belgium Government bonds (3); Brazil Government Unified (5); Canada Province of Ontario; Chile Internal Government (7); China Internal Loans; Colombia Internal Government (7); Czechoslovakia Government Bonds (miscellaneous); Denmark Perpetual Government Bonds (3.5); Egypt Government (3.5); Finland State Loans (miscellaneous); France Irredeemable Government Bonds (3); Germany Public Bonds (6 until 1935, then 4.5); Greece Refuge Loan (8); Hungary Forced Loan 1924 (5); India State Loan (4); Italy Government Bond (3.5); Japan Government Bonds (miscellaneous: average of public bonds); Netherlands Irredeemable Government bonds (2.5-3); New Zealand Government Bonds (4); Norway Miscellaneous Bonds (4.5); Poland 1919/20 Dollar Loan (6); Romania Government Bonds (miscellaneous); Spain Internal Debt (4); South Africa Inscribed (5); Sweden Government Bonds (3.25); Switzerland State and Federal Railways (miscellaneous); United Kingdom Consoles (2.5); United States Treasury (miscellaneous: average of all outstanding not callable for 12 years or more); Yugoslavia 1921 Loan (7).

Tax revenue categorization: I re-categorize the data in the original sources as follows.

Income (direct) taxes= taxes on income, earnings and capital. Indirect taxes=taxes on capital transactions and transportation+ taxes on sales+ taxes on consumption; Custom duties; Non divisible taxes.

Tax revenues and composition at central and local level: This data from is from various publications of the German Imperial Statistical Office, and is complemented by data from Moody's investment agency, the League of Nations and Ritschl (2002b). All data is in local currency. As a general rule, when the sources overlap and disagree, I use data from the later source. In order to minimize issues of reverse causality, when fiscal years do not correspond to calendar years I assign data from the year in which the fiscal end to the whole calendar year. For example, if the fiscal year ends in June 1924, I assign data for July 1923 to June 1924 to 1924. This inevitably introduces some noise in the data, but is preferable to increasing the risk of the tax data influencing the regressors. The data all refer to realized revenues rather than budgets. Data coverage could be expanded by using budgets, but these were notoriously unreliable in the interwar period, especially in poorer countries. The sources with the corresponding page numbers are: Statistisches Reichsamt (1930a) page 844-847, Statistisches Reichsamt (1924/25) page 349, Statistisches Reichsamt (1930b) Statistisches Reichsamt (1932) page 188*-189*, Statistisches Reichsamt (1933) page 214*-215*, Statistisches Reichsamt (1934) page 232*-233*, Statistisches Reichsamt (1936b) page 256*-257*, Statistisches Reichsamt (1937) page 270*-271*, Statistisches Reichsamt (1938) page 274*-283*, Statistisches Reichsamt (1939/40) page 310*-311*, Moody's (1933) and League of Nations (1936/37) page 264-273. The country by country breakdown of the sources is as follows.

Austria: central – 1925-27 Statistisches Reichsamt (1930a), 1928-30 Statistisches Reichsamt (1932), 1931-34 Statistisches Reichsamt (1937); local (states and local communities) – Statistisches Reichsamt (1933), 1931-34 Statistisches Reichsamt (1937).

Belgium: central – 1913, 1925, 1927 Statistisches Reichsamt (1930a), 1926, 1928-1935 Statistisches Reichsamt (1937); 1936 Statistisches Reichsamt (1938); 1937 Statistisches Reichsamt (1939/40); local – 1926, 1928-35 Statistisches Reichsamt (1937).

Bulgaria: central 1914, 1925-28 Statistisches Reichsamt (1930a); 1929-30 Statistisches Reichsamt (1932), 1932 Statistisches Reichsamt (1937), 1934-37 Statistisches Reichsamt (1938), 1937 Statistisches Reichsamt (1939/40); local – 1932 Statistisches Reichsamt (1937).

Czechoslovakia: central – 1926 Statistisches Reichsamt (1930a); 1927-28 Statistisches Reichsamt (1932); 1934 Statistisches Reichsamt (1936b), 1935-36 Statistisches Reichsamt

(1938).

Denmark: central – 1914, 1927 Statistisches Reichsamt (1930a); 1926, 1928, 1930-34 Statistisches Reichsamt (1937); 1936-37 Statistisches Reichsamt (1939/40); local – 1926, 1928, 1930-34 Statistisches Reichsamt (1937).

Estonia: central – 1925, 1927-30 Statistisches Reichsamt (1934).

Finland: central – 1925-27 Statistisches Reichsamt (1930a), 1928-29 Statistisches Reichsamt (1932), 1930-31 Statistisches Reichsamt (1933), 1932 Statistisches Reichsamt (1934) , 1934-35 Statistisches Reichsamt (1938), 1936-37 Statistisches Reichsamt (1939/40).

France: central – 1912, 1925-27 Statistisches Reichsamt (1930a), 1928-30 Statistisches Reichsamt (1932), 1931-32 Statistisches Reichsamt (1934), 1933-35 Statistisches Reichsamt (1937), 1936 Statistisches Reichsamt (1938); local (departments and municipalities) 1929-30 Statistisches Reichsamt (1932), 1931-32 Statistisches Reichsamt (1934), 1933-35 Statistisches Reichsamt (1937).

Germany: central – revenue only 1921-24 Statistisches Reichsamt (1924/25) , 1925-29 Statistisches Reichsamt (1930b), revenue only 1930-38 Ritschl (2002b) Tabelle A.1, composition 1936 Statistisches Reichsamt (1936b); local (States, municipalities and hanseatic cities) – revenue only 1926-38 Ritschl (2002b) Tabelle A.10.

United Kingdom: central – 1914, 1926-28 Statistisches Reichsamt (1930a), 1929-31 Statistisches Reichsamt (1932), 1932 Statistisches Reichsamt (1933), 1933 Statistisches Reichsamt (1934), 1934-35 Statistisches Reichsamt (1937), 1936-37 Statistisches Reichsamt (1938), 1938 Statistisches Reichsamt (1939/40); local – 1930-31 Statistisches Reichsamt (1932), 1932-33 Statistisches Reichsamt (1934), 1934-34 Statistisches Reichsamt (1937).

Greece: central – 1929-30 Statistisches Reichsamt (1932), 1931 Statistisches Reichsamt (1933), 1933-34 Statistisches Reichsamt (1936b), Statistisches Reichsamt (1938).

Hungary: 1913, 1927-31 Statistisches Reichsamt (1933), 1932-35 Statistisches Reichsamt (1937), 1936 Statistisches Reichsamt (1939/40); local (counties, cities and municipalities) 1929-31 Statistisches Reichsamt (1933), 1932-35 Statistisches Reichsamt (1937).

Ireland: central – 1929-31 Statistisches Reichsamt (1932), 1932 Statistisches Reichsamt (1934), 1934 Statistisches Reichsamt (1936b), 1935-37 Statistisches Reichsamt (1939/40).

Italy: central – 1914, 1926-28 Statistisches Reichsamt (1930a), 1929-31 Statistisches Reichsamt (1932), 1932-33 Statistisches Reichsamt (1937), 1934 Statistisches Reichsamt (1936b), 1936-37 Statistisches Reichsamt (1939/40); local (provinces and municipalities) 1928 Statistisches Reichsamt (1932) and Statistisches Reichsamt (1937), 1932-33 Statistisches Reichsamt (1937).

Latvia: central – 1926, 1928, 1930-35 Statistisches Reichsamt (1937), 1927, 1929 Statistisches Reichsamt (1933); local – 1926, 1928, 1930-35 Statistisches Reichsamt (1937).

Lithuania: central – 1925-31 Statistisches Reichsamt (1933), 1932 Statistisches Reichsamt (1934), 1933-34 Statistisches Reichsamt (1936b),, 1935-36 Statistisches Reichsamt (1938), 1937 Statistisches Reichsamt (1939/40).

Netherlands: central – 1913, 1926 Statistisches Reichsamt (1930a), 1925, 1927, 1929-34 Statistisches Reichsamt (1937), 1935 Statistisches Reichsamt (1938), 1936 Statistisches Reichsamt (1939/40); local – 1925, 1927, 1929-34 Statistisches Reichsamt (1937).

Norway: central – 1914, 1927 Statistisches Reichsamt (1930a), 1926, 1928, 1930-35 Statistisches Reichsamt (1937), 1936 Statistisches Reichsamt (1938), 1937 Statistisches Reichsamt (1939/40); local – 1926, 1928, 1930-35 Statistisches Reichsamt (1937).

Poland: central – 1925, 1927-28 Statistisches Reichsamt (1930a), Poland 1929-30 Statistisches Reichsamt (1932), 1931-32 Statistisches Reichsamt (1934), 1933-34 Statistisches Reichsamt (1937), 1935-36 Statistisches Reichsamt (1938), 1937 Statistisches Reichsamt (1939/40); local – 1933-34 citetReichsamt37.

Romania: central – 1928 Statistisches Reichsamt (1932), 1929-30 Statistisches Reichsamt (1933), 1934 Statistisches Reichsamt (1936b), 1935-36 Statistisches Reichsamt (1938), 1937 Statistisches Reichsamt (1939/40).

Spain: central – 1914,1925-27 Statistisches Reichsamt (1930a), 1928-29 Statistisches Reichsamt (1933), 1932-33 Statistisches Reichsamt (1936b).

Sweden: central – 1913, 1927 Statistisches Reichsamt (1930a), 1926, 1928-33 Statistisches Reichsamt (1937), 1935 Statistisches Reichsamt (1936b), 1936-37 Statistisches Reichsamt (1938), 1938 Statistisches Reichsamt (1939/40); local – 1926, 1928-33 Statistisches Reichsamt (1937).

Switzerland: central – 1913, 1925-27 Statistisches Reichsamt (1930a), 1928-30 Statistisches Reichsamt (1932), 1931-33 Statistisches Reichsamt (1937), 1934-1937 Statistisches Reichsamt (1939/40); local (cantons and municipalities) – 1925-30 Statistisches Reichsamt (1934), 1931-33 Statistisches Reichsamt (1937).

Yugoslavia: central – 1929-20 Statistisches Reichsamt (1932), 1931-33 Statistisches Reichsamt (1934).

Argentina: central – 1925-34 Statistisches Reichsamt (1936b), 1935-37 Statistisches Reichsamt (1939/40).

Bolivia: central – 1925-31 Statistisches Reichsamt (1936b), 1932-35 Statistisches Reichsamt (1938).

Brazil: central – revenue only 1927-29 percentage changes Moody's (1933), 1930-37 Statistisches Reichsamt (1939/40).

Chile: central – 1926-27, 1929-30 Statistisches Reichsamt (1933), 1931-32 Statistisches Reichsamt (1934), 1934-36 Statistisches Reichsamt (1938), 1937 Statistisches Reichsamt (1939/40).

Colombia: central – 1925-30 Statistisches Reichsamt (1934), 1934-36 Statistisches Reichsamt (1938), 1937 Statistisches Reichsamt (1939/40).

Peru: central – 1925-32 Statistisches Reichsamt (1936b), 1933-36 Statistisches Reichsamt (1939/40).

Australia: central – 1914 Statistisches Reichsamt (1930a), 1926-31 Statistisches Reichsamt (1934), 1933-34 Statistisches Reichsamt (1936b), 1935-37 Statistisches Reichsamt (1938), 1938 Statistisches Reichsamt (1939/40).

Japan: central – 1914.1926-28 Statistisches Reichsamt (1930a), 1929-30 Statistisches Reichsamt (1932),1931-33 Statistisches Reichsamt (1934), 1934 Statistisches Reichsamt (1936b), 1935-36 Statistisches Reichsamt (1938), 1937-38 Statistisches Reichsamt (1939/40).

Canada: central – 1925-28 Statistisches Reichsamt (1930a), 1929-30 Statistisches Reichsamt (1932), 1931 Statistisches Reichsamt (1933), 1933-34 Statistisches Reichsamt (1936b), 1935 Statistisches Reichsamt (1938).

Egypt: central – 1913, 1926-31Statistisches Reichsamt (1933), 1935 Statistisches Reichsamt (1938), 1936-37 Statistisches Reichsamt (1939/40).

South Africa: central – 1926-31 Statistisches Reichsamt (1933), 1932 Statistisches Reichsamt (1934), 1934 Statistisches Reichsamt (1936b), 1935-36 Statistisches Reichsamt (1936b), 1937 Statistisches Reichsamt (1939/40).

USA: central – 1914, 1926, 1928 Statistisches Reichsamt (1930a), 1927 Statistisches Reichsamt (1937), 1929-31 Statistisches Reichsamt (1932), 1932 Statistisches Reichsamt (1933), 1933 Statistisches Reichsamt (1934), 1935-37 Statistisches Reichsamt (1938).

Turkey: central – 1925-27 Statistisches Reichsamt (1930a), 1928-30 Statistisches Reichsamt (1932), 1933 Statistisches Reichsamt (1936b), 1934 Statistisches Reichsamt (1938), 1935 Statistisches Reichsamt (1939/40).

India: central – 1926-30 Statistisches Reichsamt (1933), 1931 Statistisches Reichsamt (1934), 1934-35 Statistisches Reichsamt (1936b), 1936 Statistisches Reichsamt (1938), 1937 Statistisches Reichsamt (1939/40).

Government financing and expenditure at central and local level: data govern-

ment financing and expenditure also comes from publications of the German Imperial Statistical Office. All data is in local currency. As discussed in the paper, government financing is a composite of tax revenues, non-tax revenues and long-term (over 1 year maturity) borrowing, essentially it is the budgeted part of government revenues. Non-tax revenues include the profits of publicly owned corporations, public monopolies and unclassified revenues (such as the the sale abroad of certain commodities). As is the case with most public sector data in the interwar period, accounting standards tend to differ across countries As for tax revenues, when fiscal years do not correspond to calendar years I assign data from the year in which the fiscal end to the whole calendar year and the data all refer to realized revenues rather than budgets. The authors of the yearbooks made an effort to make the data more easily comparable across countries, but issues remain (e.g. in Spain short term borrowing cannot be separated out). In the analysis, the problem is minimized by exploiting the time-series rather that cross sectional variation in the data. For instances where only some local bodies are included for some years (provinces are included, but not municipalities), I use the information from years where data is available for all local bodies to estimate the missing revenue using the average ratio between central and local revenues. The common sources with page numbers are: Statistisches Reichsamt (1924/25) page 126* Statistisches Reichsamt (1935) page 229*-235*, Statistisches Reichsamt (1937), Statistisches Reichsamt (1936b), Statistisches Reichsamt (1938) page 274*-283*, Statistisches Reichsamt (1939/40) Statistisches Reichsamt (1941/42). The country by country sources are as follows.

Austria: central financing & expenditure – 1923-26 Statistisches Reichsamt (1936a) page 208, 1927-35 Statistisches Reichsamt (1937); local financing & expenditure (states and municipalities with population over 5000)– 1927-31 Statistisches Reichsamt (1935).

Belgium: central financing & expenditure – 1912 Statistisches Reichsamt (1924/25), 1920-33 Statistisches Reichsamt (1936a) page 43, 1936 (revenue only) Statistisches Reichsamt (1938), 1937 (revenue only) Statistisches Reichsamt (1941/42); local financing & expenditure (provinces and municipalities) – 1927-32 (municipalities only) Statistisches Reichsamt (1935), 1935-36 (municipalities only) Statistisches Reichsamt (1939/40), both integrated with proportions between provinces and municipalities from 1932-35 Statistisches Reichsamt (1938) .

Bulgaria: central central financing & expenditure – 1920–33 Statistisches Reichsamt (1936a) page 55, 1935-36 Statistisches Reichsamt (1938); local financing & expenditure (departments and municipalities) – 1928-33 Statistisches Reichsamt (1935), 1934-35 Statistisches

Reichsamt (1938).

Czechoslovakia: central financing & expenditure – 1925-34 Statistisches Reichsamt (1936b), 1935-36 Statistisches Reichsamt (1937); local financing & expenditure – states and municipalities 1929-30 Statistisches Reichsamt (1935), only states 1935-38 Statistisches Reichsamt (1938).

Denmark: central financing & expenditure – 1912-13 Statistisches Reichsamt (1924/25), 1920-33 Statistisches Reichsamt (1936a) page 63, 1936 (revenue only) Statistisches Reichsamt (1938), 1938 (revenue only) Statistisches Reichsamt (1939/40); local financing & expenditure (municipalities) – 1920-27 estimate based on proportion between central and local in 1928-33 Statistisches Reichsamt (1935), 1934-36 Statistisches Reichsamt (1938).

Estonia: central financing & expenditure – 1926-27, 1933 Statistisches Reichsamt (1936b), 1928–32 Statistisches Reichsamt (1935), 1934 Statistisches Reichsamt (1937), 1937-38 (revenue only) Statistisches Reichsamt (1938); local financing & expenditure – 1928-33 Statistisches Reichsamt (1935).

Finland: central financing & expenditure – 1911-13 Statistisches Reichsamt (1924/25), 1920-33 Statistisches Reichsamt (1936a) page 84, 1934-35 Statistisches Reichsamt (1936b), 1936 (revenue only) Statistisches Reichsamt (1938), 1937 (revenue only)Statistisches Reichsamt (1939/40); local financing & expenditure (cities and rural municipalities) – 1925-31 Statistisches Reichsamt (1936a), 1932 Statistisches Reichsamt (1935), 1933-35 Statistisches Reichsamt (1938).

France: central financing & expenditure – 1911-13, 1920 Statistisches Reichsamt (1924/25), 1921-33 Statistisches Reichsamt (1936a) page 95, 1937 (revenues only) Statistisches Reichsamt (1941/42); local financing & expenditure (departments) – 1920-30 Statistisches Reichsamt (1936a) page 96, 1934 (revenues only) Statistisches Reichsamt (1939/40).

Germany: central & expenditure – 1925 Statistisches Reichsamt (1924/25), 1926-38 Ritschl (2002b); local financing & expenditure (states and municipalities) – 1926-34 Statistisches Reichsamt (1936a) page 18.

United Kingdom: financing & expenditure – 1912-14 Statistisches Reichsamt (1924/25), 1921-26 Statistisches Reichsamt (1936a) page 124, 1927-37 Statistisches Reichsamt (1937), 1938 (revenue only) Statistisches Reichsamt (1939/40); local financing & expenditure (England and Wales only) – 1921-33 Statistisches Reichsamt (1936a), 1934-35 Statistisches Reichsamt (1938), 1936 (revenues only), 1937-38 (expenditure only) Statistisches Reichsamt (1939/40).

Greece: central financing & expenditure – 1928-34 Statistisches Reichsamt (1935), 1935-36

(revenue only) Statistisches Reichsamt (1938), 1938 (revenue only) Statistisches Reichsamt (1939/40); local financing & expenditure – 1928-33 Statistisches Reichsamt (1935), 1937-38 (revenue only) Statistisches Reichsamt (1939/40).

Hungary: central financing & expenditure – 1925-26 Statistisches Reichsamt (1936a) page 305, 1927-36 Statistisches Reichsamt (1937), 1938 (revenue only) Statistisches Reichsamt (1939/40).

Ireland: central financing & expenditure – 1922-34, 1935-36 (revenue only) Statistisches Reichsamt (1936a), page 140, 1935 (expenditure only) Statistisches Reichsamt (1936b), 1938 (revenue only) Statistisches Reichsamt (1939/40); local financing & expenditure (municipalities) – 1929-33 Statistisches Reichsamt (1935).

Italy: central financing & expenditure – 1912-14 Statistisches Reichsamt (1924/25), 1921-34 Statistisches Reichsamt (1936a) page 145 1936-38 Statistisches Reichsamt (1938); local financing & expenditure (municipalities) – 1928 Statistisches Reichsamt (1935), 1935 Statistisches Reichsamt (1938).

Latvia: central financing & expenditure – 1927, 1934-35 Statistisches Reichsamt (1937), 1928-33 Statistisches Reichsamt (1935); local financing & expenditure – 1928-33 Statistisches Reichsamt (1935), 1935-37 1928-33 Statistisches Reichsamt (1938)

Lithuania: central financing & expenditure – 1926, 1932-1936 1928-33 Statistisches Reichsamt (1937), 1937 (revenues only) 1928-33 Statistisches Reichsamt (1938); local financing & expenditure – 1927-33 Statistisches Reichsamt (1935).

Netherlands: central financing & expenditure – 1911-13 Statistisches Reichsamt (1924/25), 1920-25 Statistisches Reichsamt (1936a) page 185, 1926-35 Statistisches Reichsamt (1937), 1937 (revenues only) Statistisches Reichsamt (1941/42); local financing & expenditure (provinces and municipalities) – 1927-32 Statistisches Reichsamt (1935), 1933-35 Statistisches Reichsamt (1938).

Norway: central financing & expenditure – 1921-26 Statistisches Reichsamt (1936a) page 198, 1927-35 Statistisches Reichsamt (1937). 1936 (revenues only) Statistisches Reichsamt (1938); local financing & expenditure (municipalities) – 1925-31 (revenues) Statistisches Reichsamt (1936a),1928-31 (expenditure), 1932 Statistisches Reichsamt (1935), 1934-36 Statistisches Reichsamt (1938).

Poland: central financing & expenditure – 1922-31 (revenues) 1922-28 (expenditure) Statistisches Reichsamt (1936a) page 222, 1932-37 (revenues) 1929-37 (expenditure) Statistisches Reichsamt (1937); local financing & expenditure (municipalities) – 1928-34 Statistisches Reichsamt (1935), 1936 Statistisches Reichsamt (1938).

Romania: central financing & expenditure – 1926-31, 1933-37 Statistisches Reichsamt (1937).

Spain: central financing & expenditure – 1927-34 Statistisches Reichsamt (1935), 1935 Statistisches Reichsamt (1936b); local financing & expenditure – 1927-31 Statistisches Reichsamt (1935).

Sweden: central financing & expenditure – 1911-13 (revenues) 1911-12 (expenditure) Statistisches Reichsamt (1924/25), 1920-26 Statistisches Reichsamt (1936a) page 261, 1927-34, 1936 Statistisches Reichsamt (1937), 1935 Statistisches Reichsamt (1936b); local financing & expenditure – 1921-32 Statistisches Reichsamt (1936a), 1933-34 Statistisches Reichsamt (1938).

Switzerland: central financing & expenditure – 1911-1913 Statistisches Reichsamt (1924/25), 1920-25 Statistisches Reichsamt (1936a) page 273, 1926-36 Statistisches Reichsamt (1937) except revenue 1936 Statistisches Reichsamt (1938); local financing & expenditure – 1920-33 Statistisches Reichsamt (1936a).

Yugoslavia: central – financing & expenditure 1928-32 (revenue) 1928-33 (expenditure) Statistisches Reichsamt (1935), 1933-38 (revenue) 1934-38 (expenditure) South-Eastern European Monetary Network (2014); local financing & expenditure – 1929-33 (revenue) 1928-33 (expenditure) Statistisches Reichsamt (1935), 1935-36 Statistisches Reichsamt (1938).

Argentina: central financing & expenditure – 1920-33 Statistisches Reichsamt (1936a) page 416, 1935-36 Statistisches Reichsamt (1939/40).

Bolivia: central financing & expenditure – 1920-33 Statistisches Reichsamt (1936a) page 424.

Brazil: central financing & expenditure – 1920-33 Statistisches Reichsamt (1936a) page 427, 1935 Statistisches Reichsamt (1939/40), 1936 Statistisches Reichsamt (1937); local financing & expenditure (states) – 1920-32 Statistisches Reichsamt (1936a).

Chile: central financing & expenditure – 1927-34 Statistisches Reichsamt (1935), 1937 Statistisches Reichsamt (1939/40); local financing & expenditure – 1929-33 Statistisches Reichsamt (1935).

Colombia: central financing & expenditure – 1920-33 Statistisches Reichsamt (1936a) page 466, 1936-37 Statistisches Reichsamt (1939/40); local financing & expenditure – 1927-33 Statistisches Reichsamt (1936a).

Peru: central financing & expenditure – 1920-33 Statistisches Reichsamt (1936a) page 482, 1935-36 (revenues only) Statistisches Reichsamt (1939/40), 1937 (revenues only) Statistis-

ches Reichsamt (1941/42).

Uruguay: central financing & expenditure – 1923-31 Statistisches Reichsamt (1936a) page 490, 1935-36 (revenues only) Statistisches Reichsamt (1939/40).

Australia: central financing & expenditure – 1912-14 Statistisches Reichsamt (1924/25), 1921-25 Statistisches Reichsamt (1936a) page 526, 1926-36 Statistisches Reichsamt (1936b). 1937-38 (revenues only) Statistisches Reichsamt (1939/40); local financing & expenditure (states and municipalities) – 1921-34 (municipalities), 1937-38 Statistisches Reichsamt (1939/40), both integrated with 1928-33 Statistisches Reichsamt (1935) to calculate states and municipalities.

New Zealand: central financing & expenditure – 1920-26, 1928-34 Statistisches Reichsamt (1936a) page 536, 1938 (revenues only) Statistisches Reichsamt (1939/40).

Japan: central financing & expenditure – 1911-13 Statistisches Reichsamt (1924/25), 1921-34 Statistisches Reichsamt (1936a) page 377-78, 1935 (revenues) Statistisches Reichsamt (1936b), 1936 (revenues) Statistisches Reichsamt (1938), 1935-37 Statistisches Reichsamt (1937) ; local financing & expenditure – 1921-34 Statistisches Reichsamt (1936a) page 377-78.

Indonesia: central financing & expenditure – 1920-33 (1932 and 1933 are budget figures) Statistisches Reichsamt (1936a) page 389.

Canada: central financing & expenditure – 1912-14Statistisches Reichsamt (1924/25), 1921-34 Statistisches Reichsamt (1936a) page 454, 1935 Statistisches Reichsamt (1936b), 1936 Statistisches Reichsamt (1937), 1937 (revenues only) Statistisches Reichsamt (1939/40); local financing & expenditure (provinces and municipalities) – 1920-33 (provinces only) Statistisches Reichsamt (1936a) integrated with 1928-33 Statistisches Reichsamt (1935).

Egypt: local financing & expenditure – 1937-38 Statistisches Reichsamt (1939/40).

South Africa: central financing & expenditure – 1926-27, 1934 Statistisches Reichsamt (1936b), 1928-33 Statistisches Reichsamt (1935), 1935 Statistisches Reichsamt (1937), 1937 Statistisches Reichsamt (1941/42); local financing & expenditure (provinces and municipalities) – 1928-32 Statistisches Reichsamt (1935).

USA: central financing & expenditure – 1912-14, 1921-24 Statistisches Reichsamt (1924/25), 1926-27, 1933 (revenues), 1935 Statistisches Reichsamt (1936b), 1928-32 (revenues) 1928-34 (expenditure) Statistisches Reichsamt (1937), 1934 (revenues) 1936 (expenditure), 1937 Statistisches Reichsamt (1937), 1936, 1938 (revenues) Statistisches Reichsamt (1938); local financing & expenditure (states) – 1928-32 Statistisches Reichsamt (1935).

Turkey: central financing & expenditure – 1926-27 Statistisches Reichsamt (1936b), 1928-

33 (revenues), 1928-32 (expenditure) Statistisches Reichsamt (1935), 1934-36 (revenues) 1933-36 (expenditure) Statistisches Reichsamt (1937), 1938 Statistisches Reichsamt (1939/40); local financing & expenditure (villages and municipalities) – 1928-31Statistisches Reichsamt (1935).

India: central financing & expenditure – 1926-27, 1933-35 Statistisches Reichsamt (1936b), 1928-32 Statistisches Reichsamt (1935), 1936 Statistisches Reichsamt (1937), 1937-38 (revenues only) Statistisches Reichsamt (1939/40); local financing & expenditure – 1928-32 (provinces, districts, local corporations and cities), 1933-34 (provinces) Statistisches Reichsamt (1935),1935 (provinces), 1936 (provinces and municipalities), 1937 (provinces) (revenues only) Statistisches Reichsamt (1939/40).

Chapter 5

Paper 3: Slavery, Fiscal Capacity and Public Goods Provision in Brazil: Evidence from Rio de Janeiro and São Paulo, 1836-1912

5.1 Introduction

This paper studies the effect of slavery on the development of fiscal institutions at the local level in Brazil. I focus on the two key Southeastern provinces/states of Rio de Janeiro and São Paulo and show that a high incidence of slavery in the 19th century was causally associated with lower public revenue and expenditure per capita and worse public goods provision at the municipal level in the early 20th century. My analysis indicates that slavery interacted with and helped to shape the deep structural changes that affected Brazil starting from the mid-19th century, and thus helped to define the attributes of local institutions in this crucial period. These then persisted as local governments gained prominence in Brazil's Old Republic (1891-1930). This mechanism helps to explain the large degree of variation in fiscal capacity and public goods provision within Brazil in the early 20th century.

In the analysis, I find that standard regressions are inconclusive regarding the effect of slavery on developmental outcomes, in line with previous research. This reflects serious challenges to causal inference due to the spillover effects in fiscal decisions, measurement error, and the fact that booming areas attracted large numbers of slaves and were also able to increase public revenues and expenditure. I overcome these issues by employing

spatial models and an instrumental variable strategy based on finely measured geographical indicators. I then use the location of immigrant settler colonies to investigate one of the potential channels through which slavery affected fiscal development. I find compelling evidence that slavery's impact was at least partially due to its negative influence on the establishment of foreign immigrants, who were a major force in the expansion of the public sector in Southeastern Brazil, where they settled en masse in the late 19th early 20th century. This is because migrants, unlike slaves, had a political voice, were used to European levels of public goods provision and could "vote with their feet".[61]

Brazil represents an ideal testing ground to improve our understanding of both the historical origins of fiscal institutions and the effect of slavery on economic and institutional development for various reasons. First, the country was the largest receiver of forced migrants during the Atlantic slave trade, with nearly 5 million Africans transported across the ocean between the 16th and 19th century (Klein and Luna, 2010). Second, despite significant improvements in recent decades, Brazil today is still characterized by an overall weak institutional environment and low fiscal capacity. Tax revenue as a share of output is in line with, if not above, that of countries with a comparable level of economic development. However, income taxes – a strong indicator of fiscal capacity – represent only a small share of this revenue. Brazil is furthermore plagued by corruption and tax evasion, as well as inefficient and wasteful public spending, features which are strongly connected with institutional weakness.[62] Finally, the country is characterized by substantial intra and inter-regional differences in economic outcomes – such as income, education and inequality – and in the quality of governance and institutions.[63]

Brazil's woes with fiscal and state capacity have a long history. The small fiscal resources commanded by all levels of government – national, provincial/state and municipal – and the connected poor provision of public goods were a topic of discussion amongst policy makers and commentators already in the 19th century (Nunes Leal, 1977).[64] In fact, there is ample evidence that low public spending acted as a constraint for growth due to the suboptimal provision of infrastructure and education in the mid and late 1800s

[61] Moreover, migrant communities often built their own schools for which the government would cover at least part of the costs (de Carvalho Filho and Colistete, 2010).

[62] See Shleifer and Vishny (1993) on the ties between state weakness and corruption.

[63] See Reis (2017) for a historical perspective on spatial income inequality in Brazil and Alston, Melo, Mueller, and Pereira (2016) for an overview of the progress made by Brazil in some of these areas in the last two decades.

[64] See also Abreu and do Lago (2001) for a survey of Brazilian fiscal history since colonization and Summerhill (2015) for an excellent account of public finances and borrowing at the national level in Imperial Brazil.

(Leff, 1997; Summerhill, 2005). The inadequate financing of local governments has been an issue of particular importance in Brazil. This was true not only in the 19th century, but also over the course of the 1900s, particularly after the fiscal and administrative centralization, which followed the revolution of 1930. The paltry resources commanded by municipalities played an important role in the low provision of public goods, particularly primary education, in this latter period (Kang, 2017). Up to this day, the fiscal capacity of local governments matters for the de facto provision of public services. Gadenne (2017) finds that Brazilian municipalities which expand their revenues through taxation, as opposed to grants from the federal government, are more likely to spend them on productive public goods.

With its exploration of the role of slavery in the development of local fiscal institutions, this paper relates to two main strands of literature. First, it speaks to the growing literature on state capacity. Understanding the processes through which polities acquire the capacity to tax and perform the basic tasks of modern states is of fundamental importance in order to shed light on the ways in which public institutions can support or stifle economic growth and development.[65] In line with much of the work in this field, I focus on fiscal capacity. This is because, apart from being the most easily measurable element of state capacity, fiscal capacity plays a pivotal role in supporting and complementing the development of other state capacities (Besley and Persson, 2009).

Although research on state and fiscal capacity is a burgeoning field, little is still known about the formation of fiscal institutions outside of Europe, particularly at the sub-national level (Hoffman, 2015; Nafziger, 2016; Koyama and Johnson, 2017). Warfare is at the heart of much of the historical literature on state formation and fiscal capacity development[66] but, whereas in Europe the incidence of large armed conflicts throughout history provided the stimulus for the creation of strong fiscal states and the expansion of modern forms of taxation, no comparable developments took place in many other parts of the world, such as Latin America (Centeno, 1997, 2002). At the same time, regional variation in fiscal institutions across and within Latin American nations are large. Thus, there is the need to go beyond the warfare-state formation nexus in order to understand these differences.

Second, this paper speaks to the literature on the economic effects of slavery. The work of Stanley Engerman and Kenneth Sokoloff has shaped much of the research in this field. The authors postulated that slavery emerged and flourished in parts of the Americas

[65] Some contributions outlining this point are Acemoglu (2005); Besley and Persson (2014); Hoffman (2015); Dincecco and Katz (2016); Bardhan (2016); Koyama and Johnson (2017).

[66] See, for example, Tilly (1975, 1990); O'Brien (2011); Besley and Persson (2010) and Dincecco and Prado (2012).

as a result of initial endowments favorable to large-scale plantation agriculture. This led to high inequality, adverse institutional evolution and relatively worse economic development compared to parts of the continent unaffected by large scale slavery (Engerman and Sokoloff, 1997, 2012). Nunn (2008b) and Bruhn and Gallego (2012) offer a systematic evaluation of this hypothesis, which yields only a partial empirical confirmation. Nunn finds a negative relationship between slavery and development, but not through the channel of large scale plantation agriculture posited by Engerman and Sokoloff. Moreover, he finds no support for inequality being the channel of persistence. Bruhn and Gallego find a link between colonial activities involving increasing returns to scale and worse economic outcomes today through the channel of less representative political institutions, but find no link between the exploitation of forced labour and development. However, single country studies have found evidence of a negative relationship between various forms of coerced labour and long-term development in Latin America, for exmaple Dell (2010) for Peru and Acemoglu, García-Jimeno, and Robinson (2012) for Colombia.

Even though Brazil was the largest importer of slaves during the Atlantic slave trade, the broad economic and institutional legacy of slavery in the country has not been firmly established. Therefore, this paper offers prima facie evidence on the negative impact of slavery on a fundamental ingredient of long-term economic development: fiscal capacity and public goods provision. Moreover, it offers a new channel through slavery had an impact, namely by shaping the settlement of foreign immigrants during the age of mass migration.

Although a number of studies have attempted, amongst other things, to investigate the link between slavery and long-term development in Brazil, their findings are largely inconclusive in this respect (Summerhill, 2010; de Carvalho Filho and Colistete, 2010; Reis, 2017). Some studies offer indirect evidence of the impact of slavery and of extractive colonial activities more generally (Naritomi, Soares, and Assunção, 2012; Musacchio, Martínez Fritscher, and Viarengo, 2014; de Carvalho, 2015), but the only paper showing evidence of a clear cut and direct legacy of slavery in Brazil is Fujiwara et al's work on the link between slavery and contemporary inequality (Fujiwara, Laudares, and Valencia Caceido, 2017). I review this literature in detail, alongside more general work on slavery in Brazil, in Section 2.3 of the literature review chapter of this Dissertation.

More broadly the paper also speaks to the literature on the determinants of long-term development. Much of the debate has been focused on identifying the ultimate cause of why some countries are rich and some are poor, with institutions and geography normally

being on either side of the debate.[67] However, it is becoming increasingly clear that both institutions and geography matter, that they can influence each other, and that each can matter more or less given a myriad of other circumstances.

The findings of this paper are very much in line with these ideas. On one side, they suggest that geography does matter, since, in addition to any direct effects of endowments on fiscal development, the allocation of slaves was determined at least in part by geographical features. However, the changing salience of geographical characteristics for the allocation of slaves across Brazilian municipalities was due to events unconnected to geography, namely the gradual decline and eventual abolition of slavery over the course of the 19th century. The delayed settlement of some areas of the country was furthermore aided by the late and slow diffusion of railways in Brazil, which, in turn, was mainly due to financial and political factors. The result of this is that frontier areas in Brazil relied less on slave labour compared to equally endowed areas closer to the coast because they were settled later. In turn, these developments helped to shape local institutions. This demonstrates that, as argued by North, Summerhill, and Weingast (2000), endowments transform themselves into political and economic outcomes through complex and non-linear mechanisms and that the same endowments need not yield the same outcomes.

The rest of the paper is organized as follows. I provide some historical background as well as information on Brazil's administrative set-up in Section 5.2. Section 5.3 outlines the empirical strategy employed the paper, Section 5.4 illustrates the data, and Section 5.5 presents the results of the analysis. Section 5.6 briefly concludes.

5.2 Background

5.2.1 Public finance in Brazil's municipalities

During Brazil's constitutional monarchy (1822-1889), which followed independence from Portugal, municipalities – Brazil's smallest administrative units – had very little autonomy. They relied on provincial governments for the approval of municipal regulations and the appointment of local functionaries. Decisions regarding local budgets also had to be approved by the provincial assembly. Indeed, despite its size, Brazil was a very centralized country that left municipalities little power or resources of their own. On top of this, the rural oligarchs, who controlled provincial and national governments, also had a strong

[67] See Nunn (2009) for a brief discussion.

influence on municipal affairs, a phenomenon known as *Coronelismo* (Nunes Leal, 1977).[68]

In the more remote areas of the country, the hold of power of the *coronels* on administrative processes and law enforcement was practically complete. The local population turned to local oligarchs offering votes in exchange for aid and protection, in a classic example of clientelism and patronage. In this context, boundaries between the public and the private tended to be blurred as public funds were used for private interests and private funds used for civic improvements. Moreover, the impersonality needed for bureaucratic efficiency was rare (da Costa, 2000). The situation was only marginally different in important coastal cities and other urban centers. There, other power groups, such as merchants and professionals, exerted influence alongside traditional elites (Woodard, 2005). Interestingly, neither the oligarchs nor their clients appointed to public administration were particularly interested in expanding the revenues of the municipalities since their main interest lay in power and authority, rather than in direct embezzlement or corruption involving public funds (Graham, 1990; Abreu and do Lago, 2001).

However, citizens were not entirely powerless. Colistete (2017) shows that, in the province of São Paulo citizens successfully lobbied the provincial government through municipal assemblies for the installation of primary schools. This took place even under very adverse conditions of isolation, poverty and illiteracy. After 1875, immigrants were particularly prominent as signatories of petitions requesting the creation of primary schools.

The financial and political position of Brazilian municipalities changed slowly, but steadily over time, particularly following the *Ato Adicional* (Additional Act) of 1834, which gave provinces legislative power over their own and municipal taxation and expenditure, on the condition that these did not interfere with those of the central government (Abreu and do Lago, 2001). In 1826, shortly after independence, Brazilian municipalities raised essentially no revenues of their own. By 1856, municipal revenues had reached around 3.3% of total public revenues and by 1885-86 they stood at 5.2% (Sokoloff and Zolt, 2007).[69] The end of the Monarchy in 1889 and the creation of a more federalist republic in 1891, led to municipalities gaining prominence. During what has become known as the Old Republic (1891-1930), mayors were elected rather than appointed in most states, and the new Constitution established that provincial and municipal ordinary budgets would have to fund primary education. Additionally, in booming regions of the country, municipalities increased their revenues rapidly thanks to growing intakes from taxes on coffee and on

[68] The term *Coronel* (colonel) in this context is not tied to the military. It is a term used to identify influential local figures and oligarchs who traditionally bought posts in the National Guard.

[69] da Costa (2000) reports a lower figure, less than 3%

activities and professions (de Carvalho Filho and Colistete, 2010).

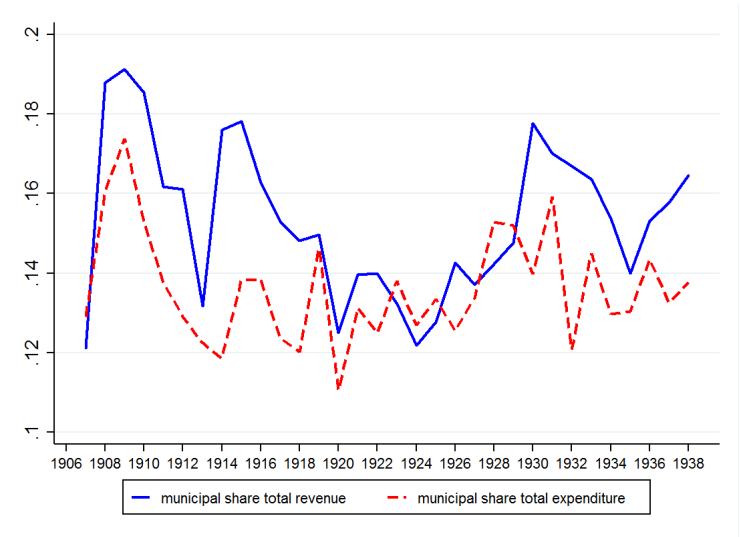
Even after these reforms, however, the distribution of public revenues across various levels of government and the scarce resources commanded by municipalities continued to be a prominent issue. In the late 19th and early 20th century, it was widely believed that Brazil's municipalities, particularly rural ones in the backlands, did not possess enough resources to provide basic services like healthcare and education to the population. There was, moreover, a feeling that large urban centres absorbed fiscal resources from these regions and thus curbed their development (Nunes Leal, 1977). In turn, the development of the backlands came to be seen as necessary for the continued industrial development of São Paulo and Rio de Janeiro through the expansion of the domestic market, making the issue salient for both central and state governments.

In any case, within a context of overall low public revenues, municipalities had become important players in the collection of tax revenue and the provision of public goods by the beginning of the 20th century. As shown in Figure 5.1, between 1907 and 1938, municipalities were responsible for around 15% of total public revenue and 14% of total public expenditure on average. At the state level, municipalities produced 24% and 23% of total revenues and expenditures respectively, employed around one third of all workers in civil administration, and were responsible for significant shares of expenditure in key areas. As an example, in 1919-23 close to 20% of public expenditure on education in some states and over 50% of average expenditure on public works were performed by municipalities.

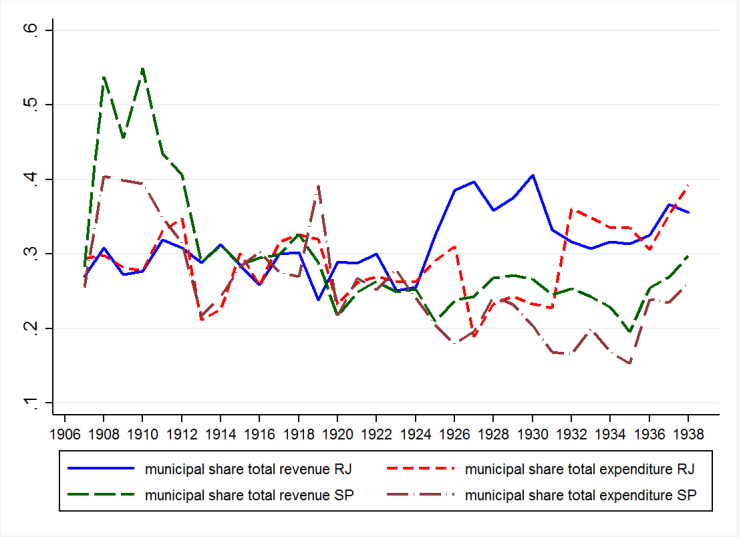
In Rio de Janeiro, municipalities were responsible for around 31% of the state's public revenue and 29% of its public expenditure between 1907 and 1938, while in São Paulo the figures were 30% and 26% respectively, higher levels than the Brazilian average. In Rio de Janeiro municipalities were responsible for 50% of public works and around 6% of education expenditure, while in São Paulo they were responsible for only 2% of public education expenditure, but still provided over 45% of public works expenditure. Moreover, notwithstanding the low expenditure on education, municipal schools in São Paulo provided around one third of school places in 1912.

5.2.2 Economic growth and structural changes in 19th and early 20th century Brazil

Brazil's economy in 19th century experienced a long period of stagnation, at least on the surface (Figure 5.2). Growth in the 1870s and 80s was later reversed, so that in 1900 Brazil's GDP per capita stood more or less at its 1800 level. Sustained growth only began



(a) All of Brazil



(b) Rio de Janeiro & São Paulo

Figure 5.1: Municipal finances in Brazil, 1907-1938

Source: Brazil (1939/40)

around the turn of the 20th century and was particularly fast between the 1930s and the late 1970s. However, the apparent stagnation of the 19th century masks deep structural changes. One of the key developments of this period was a permanent shift in the center of gravity of the Brazilian economy from the Northeastern sugar-producing regions to the Southeastern coffee-growing provinces. Although the process possibly began earlier with the discovery of gold in Minas Gerais, the coffee boom was instrumental in bringing it to fruition (Klein and Luna, 2010).

Rough estimates indicate that per capita income in the Northeast fell by 30% between 1822 and 1913. The disastrous performance of the region was driven by the poor

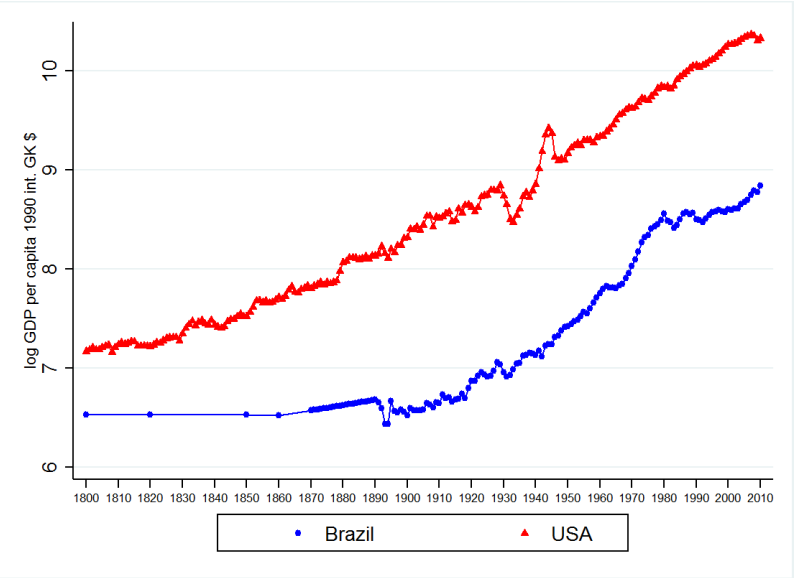


Figure 5.2: Real GDP per capita in Brazil and the USA, 1800-2010

Logarithmic scale. Source: Bolt and van Zanden (2013)

performance of the region's two main exports: cotton and sugar. While in 1822 sugar represented 49% of Brazil's exports and cofffee 19%, by 1913 cotton and sugar combined were only 3% of Brazil's exports while coffee accouted for 60% (Leff, 1997).[70]

The diffusion of railways further contributed to the deep structural changes of the 19th century. Pre-railways, goods were usually transported by mules or carts on unpaved roads stretching over uneven and mountainous terrain. This meant that ransport costs were so high as to absorb around 1/3 of the value of coffee production, for example. Inadequate transportation infrastructure thus affected the export sector, but also constrained the growth of the internal market by limiting the reach of domestic producers of non-cash crops to their immediate surroundings. The development of an internal market for manufactured goods was similarly stunted (Leff, 1997; Summerhill, 2005).

The country's first railway line was bulit in 1854, but by 1890 the network was still

[70]The expansion of coffee production in the southeast, however, did not translate into significant wage increases for workers. This was due to the elasticity of the labour market, which was fueled by the reallocation of slaves to the coffee regions on one side, and (subsidised) immigration on the other. Although the second half of 19th century is usually seen as the most dynamic one in the century due to the coffee export boom, a revisionist view has drawn attention to the post independence decades. According to this view, the shock of the abolition of slavery in the British Caribbean combined with other structural changes interacted with and facilitated Brazil's continued reliance on slave labour and expansion of the frontier (Leff, 1997) . This view is supported by a reassesment of official export statistics which have been found to be unreliable (Absell and Tena-Junguito, 2015). Leff (1997), however, warns against placing emphasis exclusively on the export economy. The majority of the population in 19th century Brazil was engaged in the domestic agricultural sector producing for local markets. In particular, most of the free population (1/2 to 2/3 of total population in 1800, around 70% in 1820) was employed outside the export sectors where, instead, slave labour was predominant. In 1911-13, exports accounted for 16% of Brazil's GDP and exports had grown faster than the rest of the economy in the 19th century, meaning that their importance was even lower in the 19th century.

limited. Substantial construction towards the interior only started during the last decade of the 19th century and by 1914, Brazil's railway expansion was equivalent to that of the US in the 1850s.[71] Private local and foreign capital, particularly British, contributed to the expansion of railways, while governments – central and local – were not nearly as active as they were, for example, in the US in the financing of transportation infrastructure (Leff, 1997; Summerhill, 1997). Nonetheless, government intervention was essential in kick-starting railway construction. This set off a virtuous cycle between infrastructure investment and public revenue growth by facilitating the growth of exports (Bignon, Esteves, and Herranz-Loncán, 2015).[72] By the turn of the 20th century, the widespread diffusion of railways meant that the growth that had been limited to the export sector expanded to the domestically oriented sectors and the internal market (Leff, 1997; Summerhill, 2005).

Rio de Janeiro and, especially, its neighbor São Paulo benefitted hugely from developments in this time period. The province of Rio de Janeiro had enjoyed a more precocious prominence compared to São Paulo as home of the colonial capital since 1723 and of independent Brazil after 1822, even though the capital city formed a separate political entity.[73] Rio de Janeiro also partook in large scale sugar cane production and, later, in the coffee boom. As a consequence of these factors, it was home to vast number of slaves, which often entered the country precisely through the port of Rio de Janeiro, especially from the 18th century onwards.

São Paulo became the economic powerhouse of Brazil at a later stage, and mainly thanks to the coffee boom that began in the 1830s. Sugar cane and cotton were cultivated in São Paulo, but the province was not an important producer of either. It was also not touched by the gold boom of the 18th century. Its first true commodity-based boom was that of coffee. By the late 19th century, however, São Paulo dominated coffee production in Brazil and Brazil dominated coffee production in the world. Around the same time, other forms of economic activity started to flourish, and by 1940 the state had become the country's most important industrial and financial centre.

Thus, the coffee boom played a big part in the structural changes that affected Brazil in the 19th century, and had lasting effects on the structure of the economy. New domestically financed and owned industries took root precisely in the regions that benefitted the most

[71] The location of railway lines was influenced by the financial and poltical power of local landowners and thus tended to follow the existing settlement of population and economic activity. Being located next to railways tended to increase the value of land, besides providing easier access to markets. This led to disputes, which potentially slowed down concessions and construction. Landowners also lobbied to keep tariffs low and promote import of machinery from abroad at low or zero tariffs (Summerhill, 1997).

[72] Similar developments took root also in many other parts of Latin America.

[73] The city remained the nation's capital until the inauguration of Brasília in 1960.

from growing exports and foreign capital inflows in the late 19th and early 20th century (Haber, 1997). Today, the Southeast is still the richest region in Brazil while the Northeast is relatively poor. There are indications that path dependency also worked at a more micro level. Monasteiro (2010) finds almost no instances of reversal of fortunes in the income per capita of Brazilian municipalities between 1872 and 2000.

5.2.3 Slavery and experiments with free labour in the 19th century

Structural changes in the economy were mirrored by changes in the structure of the population. After Independence, slaves were concentrated in the province of Rio de Janeiro, in the sugar-growing regions of the Northeast and in the gold-mining areas of Minas Gerais. However, as coffee production spread to the Southeast, this region started to acquire slaves in substantial amounts (Leff, 1997; da Costa, 2000).

Coffee growing started to expand rapidly around 1830. The main production centre in this early phase, which reached its peak around 1850, was the Paraíba Valley, which is located across the border of São Paulo and Rio de Janeiro, in the eastern part of the former and the western part of the latter (Fausto, 1999). Over time, the heartland of coffee production shifted dramatically away from the Paraíba Valley towards the north and west of São Paulo and towards Minas Gerais (Klein and Luna, 2010). By the second half of the 1800s, differences in productivity were dramatic: Paraíba valley growers produced around 20-50 *arrobas*[74] of coffee per 1000 trees compared to 80-100 in the West of São Paulo (da Costa, 2000).

Mounting pressure from Britain made the continuation of slave-based coffee production increasingly difficult as the 19th century progressed. The slave trade to British colonies was abolished in 1807 and in 1815 it became illegal north of the equator. In 1831, British insistence led to the adoption of a first law abolishing the slave trade in Brazil. This, however, had little practical consequences on the actual inflow of slaves due to the lack of a real political desire to end slavery, which played a very large role in Brazil's agrarian economy, and the inability of the government to impose its will on the oligarchs that dominated the provinces both politically and administratively. So, the slave trade continued practically unabated and actually grew in numbers as coffee plantations boosted demand for labour (da Costa, 2000). In 1850, however, the international slave trade was finally de facto abolished through the implementation of severe and effective measures against smuggling, which continued at a drastically reduced rate for a number of years. Once

[74] An arroba corresponds to 15kg.

again British influence was decisive for this outcome (Fausto, 1999).

The internal reallocation of slaves to the coffee growing regions became particularly important after the abolition of the international slave trade and remained so until the abolition of the transfer of slaves across provinces in the late 1870s. The number of slaves arriving to the Southeast from the rest of Brazil ranged between 6,000 and 9,000 a year in the 1850s and 60s (Slenes, 1975; Klein, 1978). This meant that, while in 1823 Minas Gerais, Rio de Janeiro, and São Paulo held around 386,000 slaves and the Northeastern provinces of Bahia, Pernambuco and Maranhão held around 484,000, in 1872 – when the slave population reached its peak in absolute, but by no means relative, terms – the traditional sugar growing regions held 346,000 slaves and the coffee regions approximately 800,000. Even with slave numbers in sharp decline, the slave population in the fast growing West of São Paulo grew by 15% between 1874 and 1883, while decreasing in absolute numbers in older coffee growing regions (Klein and Luna, 2010).

Notwithstanding this large internal reallocation of slaves, the huge expansion in coffee production, the high mortality rates of the slaves and the abolition of slave imports meant that slave labour became increasingly inadequate to satisfy the labour demand of the Southeast. Moreover, the *Lei do Ventre Libre* (Law of the Free Womb) of 1871 marked the beginning of a strong abolitionist movement within Brazil. Although the law had a small direct impact, since most children technically born free were forced to remain with their masters until they turned 21, there were substantial indirect effects. Self purchase by the slaves increased, third party interventions to free slaves became more common, and active legal actions by the slaves to obtain freedom were more likely to be successful. The rapid increase in abolitionist sentiment, combined with increasing slave revolts and successful escapes, eventually culminated in the abolition of slavery in 1888 (Klein and Luna, 2010).

In the face of these developments, planters and policy-makers alike started to look for alternatives to slave labour. Initial experiments with the subsidization of immigrants to form colonies were generally unsuccessful due to badly designed contracts between colonists and planters and the lack of incentives pushing the two parties towards the same goals. The province of São Paulo with its rapidly expanding coffee plantations was at the forefront of such experiments and the provincial government cooperated with private individuals to promote the settling of Europeans on Paulista plantations as early as 1829, and more vigorously from the 1840s. Initial share-cropping colonies were set up principally in the more recently settled lands of the west of the state where colonists worked alongside

slaves, albeit with distinct functions (da Costa, 2000).

By the second half of the 1850s, however, discontents were being voiced by both planters and colonists. Notwithstanding very high slave prices, due the abolishment of the slave trade, planters still relied heavily and appeared to prefer slave to free colonists' labour.[75] Although there were exception to this generally negative experience and some planters continued to experiment with and promote free labour, planters seemed generally ill-equipped to deal with the peculiarities of free labour. A common point of contention was the tendency of settlers to refuse to work when dissatisfied. And dissatisfied they tended to be, with the terms of their contracts, with their living quarters, with their confinement to the plantations, and with the tasks of repairing roads and other infrastructure, which they saw as falling outside their contracts. Moreover, Catholicism was the only recognized religion in the country, while protestantism was the chief cult amongst Swiss and German colonists. This created further frictions with the planters (da Costa, 2000). Another cause of discontent, which also reveals that both planters and the authorities intended the colonists to be a direct replacement for slaves by tying them to specific plantations, was the fact that immigrants who received subsidies to relocate to Brazil were barred from buying land for three years after their arrival (Fausto, 1999).[76] Against this backdrop, settlers abandoning the plantations with unfulfilled contracts and outright revolts became frequent (da Costa, 2000).

Although settler colonies established at a later stage are generally considered to have been more successful than these early experiments, the experience with this type of subsidized migration is not generally seen as particularly fruitful (Cameron, 1931). Sending countries were also worried about the conditions of migrants to Brazil, particularly those residing in colonies and working on coffee plantations. Growing concerns in Italy, for example, eventually led to the "Decreto Prinetti" of 1902, named after Italy's foreign minister of the time, which made subsidized emigration from Italy illegal, and was targeted mainly at migration to Brazil. Prussia nominally prohibited emigration to Brazil as early as 1859, and similar measures were implemented for the whole German empire from 1871 (Fausto, 1999).

The economic changes of the second half of the 1800s had a profound impact on free labour, however. Transportation improved, machinery started being introduced on coffee plantations and coffee prices worldwide were were genarlly high. This shifted the balance

[75] Not all colonists were perceived equally. In general, Portuguese settlers appear to have been perceived as better suited for plantation work than the other two major colonist nationalities of the time: Swiss and Germans (da Costa, 2000).

[76] This provision became effective with the approval of the 1850 Law on Land.

in favour of free labour and specialization. Initially, however, obstacles to the widespread diffusion of agricultural free labour, such as traditional sharecropping arrangements, still prevailed. This led to both the perpetuation of slavery in traditional coffee and sugar producing regions and the reallocation of more slaves from declining areas to booming ones. Only when the abolition of slavery was looming large, did interest in importing free labour become strong again. Between 1872 and 1885, 42,000 mostly Italian and Portuguese immigrants entered São Paulo. In 1886-87, 122,000 entered the province, and 800,000 more in the following decade (da Costa, 2000). Between 1885 and 1909, a total of around 2.8 million European migrants entered Brazil and the majority went to satisfy the demand for labour of coffe plantations, industries and other activities in the Southeast (Figure 5.3).[77]

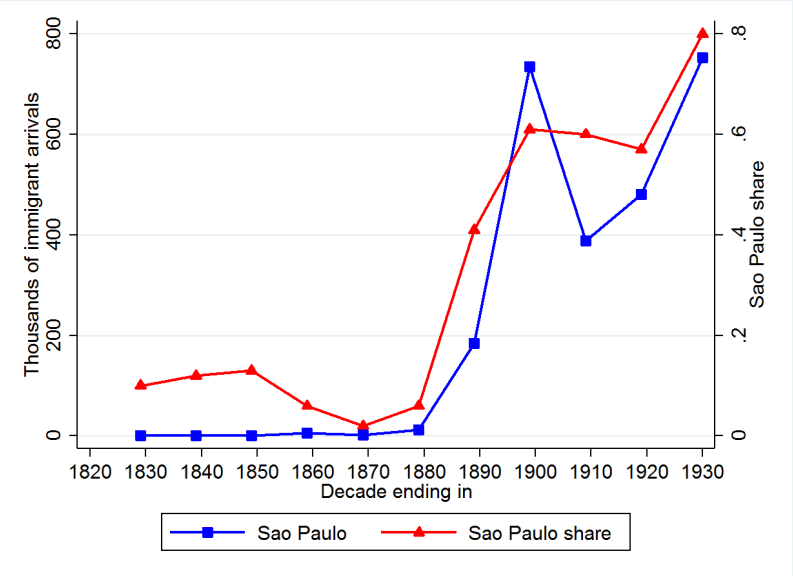


Figure 5.3: Immigrant inflow into São Paulo in absolute numbers and a share of total migration to Brazil, 1820-1930

Source: Cameron (1931)

Thus, eventually, the project of substituting slaves with immigrants was a success. This success was helped by heavy state intervention. Monetary incentives and propaganda both played a role. The virtues of migrating to Brazil were extolled in Europe via pamphlets and other publications exemplified by the magazine "O Immigrante" published for the first time in 1908 by the *Secretaria da Agricultura* (Department of Agriculture) of São Paulo in six languages: portuguese, spanish, italian, french, german and polish (Figure 5.4). Planters lobbied both the central government and the provincial governmet of São

[77] Most other immigrants settled in the southern provinces of Santa Catarina, Paraná and Rio Grande do Sul. There, they generally received land and set up their own communities instead of substituting slave labour on plantations (Leff, 1997).

Paulo to subsidize immigration. This led to the government generally fronting the cost of the voyage for the immigrants. Apart from increasing the net private return of moving to Brazil, this helped to overcome credit constraints faced by potential immigrants that might otherwise have prevented them from undertaking the trip. For a limited time, the Imperial government also payed a small daily wage to colonists. However, by 1900 push factors in Europe were so strong that the majority of immigrant arrivals were unsubsidized (Cameron, 1931).

Figure 5.4: Cover page of the first edition of the propaganda magazine "O Immigrante", published in January 1908

Source: Arquivo público do estado de São Paulo

5.3 Empirical strategy

The central goal of this paper is assessing how slavery affected the development of municipal fiscal institutions in Rio de Janeiro and São Paulo. The hypothesis is that a greater reliance on slave labour in the 19th century reduced the demand and supply of public goods and the degree of local fiscal capacity. Naturally, in order to identify slavery's impact on local institutions, this needs to have persisted after its abolition and until the early 20th century, when I observe my outcomes.

There are several potential channels through which slavery might have influenced fiscal development. First, a widespread reliance on slave labour might have heightened ethnic cleavages, making resource sharing through taxation and public goods more difficult. This

hypothesis does not appear to be supported by the data, however. I find that, after controlling for the incidence of slavery, more ethnically fragmented municipalities had consistently higher fiscal capacity and public goods provision. This result runs contrary to much of previous research, which finds a negative relationship between ethnic and linguistic fragmentation and public goods provision (Alesina, Baqir, and Easterly, 1999; Alesina and La Ferrara, 2005), but is in line with more recent work, which suggests that the effect of ethnic and linguistic fractionalization will depend, in practice, on the degree of local mixing and of cross-ethnic interactions (Desmet, Gomes, and Ortuño-Ortín, 2016). Thus, at least at the local level – see Lieberman (2003) for some more general considerations on racial relations and taxation in Brazilian history – ethnic fractionalization might have contributed to more mixing and resource sharing in Rio de Janeiro and São Paulo. The absence of racial ghettos in Brazil (Klein and Luna, 2010) reinforces this idea, which however, requires further exploration in future research.

A second channel for the link between slavery and low fiscal capacity is the fact that slaves were not educated. This reduced the need to fund public schools, and thus the need to raise public revenue. Third, a high incidence of slavery deprived a large share of the population of a political voice. Moreover, it limited the incentive of municipalities to attract workers or prevent them from emigrating by providing public services, since slaves obviously could not "vote with their feet". This also reduced the strength of free citizens' demands and the accountability of local politicians for as long as slave labour was a viable alternative to free labour. A final channel, closely related to the latter, is that the presence of slaves influenced the distribution of foreign immigrants across municipalities and that this, in turn, helped to shape local institutions.

Late 19th century migration clearly played a big role in Brazilian economic development. Stolz, Baten, and Botelho (2013), for example, find that it had positive effects on human capital and income per capita in the long term. de Carvalho Filho and Monasteiro (2012) look specifically at settler colonies and show that, in the Southern state of Rio Grande do Sul, proximity to 19th century immigrant colonies is related to better economic outcomes today. At least part of the effect of immigration is probably due to its impact on local institutions. Unlike slave labour, free labour provided a stimulus for the provision of public goods. This is because migrants, unlike slaves, could "vote with their feet" and, unlike the local population, were used to higher European levels of public goods provision. Thus, competition between municipalities keen on attracting workers from abroad presumably manifested itself in the provision of public goods, such as education healthcare, public

hygiene, sewers, public lights and public works. Migrant communities also tended to set up their own schools, which were often partially financed by public coffers (de Carvalho Filho and Colistete, 2010). In a different setting, that of pre-modern Germany, Dittmar and Meisenzahl (2016) show that local level fiscal institutions and public goods provision can indeed shape migration patterns. They find that reforms, which led to the expansion of fiscal capacity and public education, led to the production and the immigration of upper-tail human capital in cities which adopted them. Similar mechanisms might have been at work in Brazil.

Furthermore, in areas wishing to attract migrants, or where migrants settled, the paternalistic and clientelistic relationships that characterized much of Brazilian local politics – summarily known as *Coronelismo* – were less likely to thrive. This is because these relationship would need to be established anew with a different class of citizens that was geographically mobile. In the US South, landowners successfully lobbied the Federal government through their representatives to limit the growth of the welfare state. Their aim was tying the work force – ex-slaves in particular, initially – to the land by offering services within paternalistic and clientelistic relationships (Alston and Ferrie, 1985, 1993). Although different tools, such as vagrancy laws, were used in the Brazilian context to keep freed slaves tied to the plantations (Huggins, 1985; Klein and Luna, 2010; Bucciferro, 2017), similar mechanisms might have been at work in post-abolition Brazil as well. This possibility will be explored in future research.

In summary, the migrant settlement channel I propose in this paper is tightly linked to political representation. However, it is not the type of political representation embedded in the formal institutions emphasized by much of the literature – for example Acemoglu, Bautista, Querubín, and Robinson (2008) and Engerman and Sokoloff (2012)) – and analyzed by Summerhill (2010) and Funari (2017) in the Brazilian setting. These studies have focused on the extent of the political franchise and the right to vote, I argue that it was the migrants' geographical mobility and their ability to punch above their weight politically due Brazil's labour shortage that mattered. This might explain why the latter authors find no link between narrowly defined political inequality and long-term development.

As shown, there are ample reasons to believe that the inflow of immigrants stimulated local fiscal development and public goods provision. But in what way could slavery have affected the settlement of foreign immigrants? Many authors have argued that the existence of slavery stunted the development of efficient markets for free labour and mass immigration from Europe (Klein and Luna, 2010). However immigration did eventually

take place, and became thunderous already before slavery was abolished. Moreover, free labor existed before mass immigration, and the incidence of slavery was vastly different across the Brazilian territory. Therefore, it is clear that the institution of slavery affected local free labour differently in different parts of the country.

In São Paulo, the duality between slave and free labour was very salient, especially in the 1880s, when large scale immigrant inflows to the province began and slavery had not yet been not abolished. The presence of large slave populations in a municipality likely reduced the settlements of immigrants due to both a lower demand for labour and the extremely negative perception slavery had acquired by the late 19th century both in and outside Brazil. Evidence of this are the pamphlets created to attract European immigrants to Brazil by the Society for the Promotion of Immigration[78], which purposely failed to mention the existence of slavery (Fausto, 1999). It is also useful to recall that Brazil was one of the last countries to abolish the institution. Rhetoric aside, this quotation from a report of the president of the province of São Paulo to the provincial assembly in January 1889, merely 8 months after the abolition of slavery, illustrates the impact slavery had on potential immigrants to Brazil:

> *We were not known to civilized nations. Slavery made us barbarians in the eyes of foreigners and, due to ignorance or bad faith, outside of Portugal, which as a whole could not populate just one of our provinces, the idea of migrating to Brazil for those who could, if at all there, was considered terrible. It appeared as a country not habitable by civilized people, due to endemic diseases and its climate. This false judgement is now completely undone. Thousands of letters from Italians, Belgians Germans, Spaniards and individuals from other nations cross the seas, bringing to the relatives and friends that remained behind the welcome news that immigrants found in the Paulista land an adoptive home that is free and happy, where there is room for all aspirations and for all faiths, with a governmental structure modeled in line with the most civilized in the world.*[79]

[78] In Portoguese: Sociedade Promotora de Imigracão. It was founded by members of the Paulista elite keen on promoting European migration to the province in 1886.

[79] Author's translation of the relatorio apresentado á Assembléa Legislativa Provincial de São Paulo pelo presidente da provincia, dr. Pedro de Azevedo, no dia 11 de Janeiro de 1889. Jorge Steckler & Comp. "Não eramos conhecidos das nações civilsadas. A escravidão fazia-nos passar aos olhos dos estrangeiros como uns barbaros e, por ignorancia ou má fé, fóra de Portugal, que todo elle não chegaria para o povoamento de uma só de nossa Provincias, a idéa em que era tido o Brazil entre os que podîam emigrar, si não era nulla, era pessima. Figurava-se-lhes un paiz impossivel de ser habitado por gente civilisada, devido a emfermidades endemicas e ao seu clima. Esto falso juizo está desfeito interiamente. Milhares de cartas de Italianos, de Belgas, de Allemães, de Hespanhóes e de individuos de outras nações atravessam os mares,

Therefore, slavery likely affected the development of fiscal capacity and the provision of public goods both directly and indirectly. On one hand, slaves lacked the however limited political voice that free Brazilian citizens had. On the other, slavery influenced the settlement of foreign migrants and thus the extent to which this dynamic group of citizens was able to shape local institutions.

There are also good reasons to believe that the impact of slavery persisted after its abolition, and that its persistence was directly tied to its previous pervasiveness. In areas with a long-history of slave labour, such as the Paraíba valley and the state of Rio de Janeiro, ex-slaves often remained as artisans, workers in the growing industrial sector or as sharecroppers on coffee plantations . Slaves in frontier areas of São Paulo, instead, tended to follow their predecessors who had either been freed or had escaped slavery in relocating to other areas, particularly the city of São Paulo (Fausto, 1999). Thus, on frontier plantations the replacement of slaves with European immigrants took place very rapidly after abolition. At the same time, the shadow of slavery continued to affect those areas that had been most affected by it throughout the 19th century, potentially strengthening its legacy. Non-white Brazilians also continued to face discrimination and their social mobility remained limited (da Costa, 2000).

5.3.1 Threats to identification and solutions

I perform my analysis in two steps. In the first (Sections 5.5.1-5.5.4), I use the rich data sources for São Paulo in conjunction with a spatial models and an instrumental variable approach to establish the causal effect of slavery on fiscal capacity and public goods provision. In the second step (Section 5.5.5), I use data for both Rio de Janeiro and São Paulo to test the robustness of this relationship by expanding it to a setting with a significantly different history.

Importantly for the interpretation of the results, I control for the initial level of state capacity in my analysis. This is essential to avoid an obvious omitted variable bias, due to the fact pre-existing institutions might affect both my regressors and the outcome. However, it also means that I capture the effect of slavery on the development of local institutions after 1872. The total effect of slavery on fiscal capacity and public goods provision is likely to be larger. My results can thus be interpreted as lower bound estimates of the true effect of slavery. However, post-1872 developments likely capture the lion's share

levando as parentes e amigos, que ficaram, a grata noticia de que os emigrados encontraram na terra Paulista uma patria adoptiva livre e feliz, onde ha logar para todas as aspirações e para todas creanças, sob un regimen governamental modelado pelos dos mais civilisados do mundo."

of the effect of slavery, particularly in São Paulo. 1872 was the peak year in terms of slave numbers in the province (Summerhill, 2010). Moreover, the very limited independence and dearth of own revenues of municipalities in the first half of the 19th century, suggest that the late 19th century played the key role in the development of local institutions.[80]

The identification of the causal effect of slavery on fiscal capacity development and public goods provision faces several serious threats. The clearest is that some factor might be driving both the share of slaves in a municipality and the size and composition of public revenues and expenditure in that same municipality. The prosperity and/or the growth rate of the local economy might be such a factor. If rich or fast growing municipalities acquired large numbers of slaves and also expanded their public sectors, the effect of slavery on fiscal capacity would be biased upwards. Indeed, the simple OLS regressions below illustrate a positive relationship between the slave ratio and fiscal capacity indicators when only a limited set of controls is used (Table 5.1).

A straightforward solution to deal with this issue is to introduce a wide range of controls able to capture differences across municipalities related to both the incidence of slave labour and fiscal development. As the analysis below shows, this goes a long way in dismantling the idea of a positive effect of slavery on local fiscal institutions (Table 5.2). However, the regressions still do not support the existence of a negative relationship between slavery and fiscal capacity. This is plausibly due to the fact that some unobserv-

[80] As shown above, the role of municipalities in tax collection and administration became more important over the course of the Imperial period. An interesting demonstration of this is the evolution of the taxation of transactions involving slaves. The *siza* tax was applied to transactions of immobile goods. Its standard rate was 10%, but its extension to the internal slave trade was with a rate of 5% (*meia siza*). The tax concerned transactions of Brazilian-born slaves (but really this meant slaves not newly entered in Brazil) and was introduced alongside a number of new taxes, between 1801 and 1814. These were all applied uniformly across the country giving, for the first time, a degree of fiscal uniformity to the (then) Portuguese colony (Costa, 2005; Rodrigues, Craig, Schmidt, and Santos, 2015). Part of the reason for its introduction was paying for the arrival of the Portogues court in Brazil in 1808 (Abreu and do Lago, 2001; Fernandes, 2005). This tax became particularly important after the abolition of the international slave trade in 1850, both as a source of revenue for provinces and for the purpose of slowing down the transfer of slaves to the booming coffee growing regions. This was important because of the belief that an excessive transfer of slaves out of the Northeast would lead to growing support in the region for the abolition of the institution. In any case, the *Ato Adicional* of 1834 had devolved the administration of the tax to provincial governments (Costa, 2005) and the large reallocation of slaves from the Northeast to the Southeast meant that the number of transactions in these areas increased greatly. The tax was levied on the final price of slaves in the purchasing region (Klein and Luna, 2010), thus increasing the oversight of the state and of local governments, as well as their ability to tax. Additionally, from 1884 transactions involving slaves had to obligatorily go through public courts and slaves became subject to registration. These reforms were introduced in order to reduce tax evasion of both the *meia siza* and on taxes on slaves in urban areas. They appear to have been quite effective in raising fiscal revenues, but were not uniformly successful across Brazil. In Rio Grande do Sul, the changes were implemented with little resistance and further taxes on slavery were introduced, amongst other reasons, to help subsidize European immigration (Costa, 2005). In a number of states in the North, the tax was substituted with levies on slaves sold outside the provinces. Following the obligation of registration, Minas Gerais simply abolished the *meia siza*. In São Paulo, the *meia siza* was suspended for a period in 1849-50, and the registration of slaves was imperfectly implemented (Costa, 2005).

able factors might still be driving both the incidence of slave labour and the the size and allocation of public revenue and expenditure. Moreover, not all of these factors need be related to prosperity/growth – they could, for example, be related to the local colonial history – making the direction of the bias unclear.

A further threat to identification is the fact that I measure my slave ratio indicator (as well as the other variables) with noise and, as a consequence, measurement error. This is due to the creation of new municipalities between when I observe my right-hand side variables (1836 and 1872) and my outcomes (1908-12). If the measurement error were random and restricted to only one variable (i.e. classical measurement error), this would lead to attenuation bias, pushing the coefficient of the affected variable towards zero. However, neither condition for classical measurement error is likely to be met. Municipalities were clearly not created randomly and the error affects all variables measured in 1872 and 1836.

In order to reduce noise as much as possible, I identify parishes present in the 1872 census which had become municipalities by 1912 and use their own data instead of the aggregate municipal measures. I also subtract these parishes form the municipality they belonged to in 1872, further increasing the precision of my estimates. An alternative to this strategy would be to aggregate municipalities in minimum comparable areas (MCAs) with unchanged borders between the two periods of observation. This, however, would entail the loss of a large degree of variation and information, since very heterogeneous units would be averaged out in the same geographical areas. For this reason, I prefer the painstaking manual linking of municipalities over time.

Not all 1908-12 municipalities can be identified in the 1872 census. Out of São Paulo's 173 municipalities in 1912, I identify 122. The remaining 51 are assigned values from their municipality of origin. For Rio de Janeiro, data can be retrieved for 48 out of the 49 municipalities existing in 1912. For the 1836 São Paulo data, no parish level information exists, so I rely on data aggregated at the level of the 40 municipalities existing at the time. These variable thus amount to, essentially, regional, rather than municipal level controls. I further address the measurement error issue by introducing a dummy for municipalities created after 1872. This emerges as significantly negative in most specifications, indicating that younger municipalities tended to be less fiscally endowed.

In order to satisfactorily deal with both measurement error and the possibility of endogeneity for my main variable of interest, I also employ an instrumental variable strategy. This relies on finely measured geographical variables and on exploiting the relationship between the timing of the coffee boom in the different regions of São Paulo and Rio de

Janeiro and the changing availability of slave labour.

Planters in the Paraíba valley and the Central region of São Paulo started cultivating coffee on a large scale while the Atlantic Slave Trade was still in full swing; specifically during the period 1830-1850, see Section 5.2. Moreover, they could rely on the relatively abundant supply of slaves from other parts of the country, particularly the declining North-east, to complement new arrivals from Africa. This became especially important after the abolition of the Slave Trade in 1850. By this point, immigration from Europe was still in its early and largely unsuccessful stage and did not represent a valid alternative to large-scale coerced labour. Coffee planters in the frontier regions of the North and the West of the state settled into virgin and semi-virgin territory in the second half of the 19th century and the early 20th century. Unlike their predecessors, they could not rely on new arrivals of slaves. Moreover, they faced rapidly rising slave prices due to the dwindling internal supply and the ban of interprovincial transfers, which preceded abolition by approximately a decade. On the flip-side, subsidized mass immigration from Europe made up for the slack, particularly starting from the 1880s, with huge numbers of arrivals.

Thus, the timing of exploitation of land for cultivation – prevalently of coffee – determined the extent to which plantation owners could rely on slave vs free labour. In order to capture this timing, I use the interaction between the suitability of land for coffee production and distance from the port (either Rio de Janeiro or Santos, whichever one is closer to the centroid of the municipality) as an instrumental variable for the incidence of slavery. Either variable on its own could be a good indicator of the timing of the coffee boom, since more fertile and less distant areas are likely to be exploited first for cash crop production. However, both variables are extremely likely to affect fiscal capacity through channels other than reliance on slave labour, thus violating the exclusion restriction. Specifically, both variables are important determinants of production possibilities, and thus future growth and prosperity.

The interaction between the two variables, instead, captures the timing of settlement but, since distance and land suitability are introduced as separate controls, avoids the pitfalls outlined above. The variable is clearly relevant: in the analysis below I show that, as distance from the port increased, the importance of land suitability in determining the share of slaves in a municipality's population increased. Conversely, as distance from the coast decreased, the suitability of land for coffee production became less and less important in determing the allocation of slaves across municipalities. This means that, *ceteris paribus*, in areas close to the coast, which were settled early, the distribution of

slaves was scarcely affected by the fertility of land since slave labour was abundant. In areas settled later, instead, the scarcity of slave labour led to only the most fertile areas attracting large slave numbers.

Following the conceptualization of slavery provided by Fenoaltea (1984), it can be argued that, as slave prices increased and slave labour became more scarce relative to free labour, forced labour could keep its productivity edge over free labour only in tasks where violence could be used as an incentive and for tasks requiring little skill or care. This would also be compatible with the experience of other slave economies, like the Caribbean countries, and the US's *Second Middle Passage*, which involved the transfer of vast number of slaves from coastal areas to the plantations of the South in the 19th century (Berlin, 2003; Klein and Luna, 2010). The fact that transactions involving the transfer of slaves between provinces were taxed strengthens this potential mechanism, given that it further raised slave prices.

The instrument also satisfies the exclusion restrictions because, once suitability and distance from the port are controlled for, the effect of the timing of settlement should not affect fiscal capacity development and public goods provision directly, particularly conditional on the wide array of controls in my analysis.[81] Additionally, the diffusion of railways meant that, by 1912, transportation costs had fallen dramatically and distance from ports played a relatively less important role in determining production possibilities.

An alternative to using the this instrument would be to rely directly on information we have about the timing of coffee production across different regions of São Paulo, in a similar fashion to Naritomi, Soares, and Assunção (2012). This presents two issues. One is the low geographical disaggregation of this information, which would lead to very noisy and spatially autocorrelated estimates. The second is that endogenizing the timing of the exploitation of land for coffee production using geographical variables is clearly preferable to assuming that this timing was exogenous to municipal characteristics.

The final threat to identification is due to the spatial nature of the data. The error term is likely to be correlated across observations because neighboring municipalities were subject to similar shocks and because of the measurement issues outlined above. Not accounting for this would lead to incorrect standard errors and thus incorrect inference

[81] A potential worry is that the soil of lands settled early for coffee production were exhausted more extensively due to aggressive early production techniques, which were refined over time as virgin lands were settled. If this were the case, more recently settled lands could simply be more productive, potentially invalidating the IV strategy. Fortunately, the fact that the FAO data is measured for the 1960-90 period means that such developments in land quality would be captured. Thus the FAO data is actually preferable as a control for land quality compared to contemporary land surveys, which may have been successively affected by farming techniques correlated to the treatment I am attempting to capture.

regarding the existence of an effect of slavery on fiscal capacity. Additionally, municipalities' decisions regarding the size and allocation of public funds did not happen in isolation, and the existence of spillover effects between neighboring municipalities is very likely. Acemoglu, García-Jimeno, and Robinson (2015) develop this argument theoretically by showing that decisions regarding state capacity and public goods provisions in a municipality will indeed depend on the same decisions being made in neighboring municipalities. Using a network approach, the authors show that these decisions can be either strategic complements – when the presence of high capacity neighboring municipalities makes it more likely that a municipality will also choose to invest in a high degree of capacity – or strategic substitutes – when a municipality is able to free ride on other municipalities' capacity. The authors find empirical support for the complementarity channel in their study of Colombia.

In order to deal with these issues, I run a series of specifications that account for both spatial autocorrelation in the error term and for spillover effects between municipalities. Complementarity appears to be the dominant channel in the Brazilian setting as well. I find evidence of positive spillover effects of public expenditure in general and of expenditure on public education specifically between neighboring municipalities. A very plausible explanation for this result is that citizens in a municipality could demand a similar level of public goods provision to that of their neighbors against a credible threat of moving.

5.3.2 Model

The basic model I employ in my estimations is described by equation 5.1:

$$y_i = \beta_0 + \beta_1 SlaveRatio1872_i + \mathbf{x_i}\phi + c_s + \epsilon_i \tag{5.1}$$

where y is one of six outcome variables: 1) revenue per capita, 2) expenditure per capita, 3) the share of expenditure dedicated to public services, 4) public services expenditure per capita, 5) the share of public expenditure dedicated to public education, 6) public education expenditure per capita. The main variable of interest – $SlaveRatio1872$ – is the share of slaves in the total population of the municipality in 1872, while $\mathbf{x_i}$ is a vector of controls, c are state fixed effects (these are relevant when Rio de Janeiro is included in the regressions) and ϵ is the idiosyncratic error term. I run this basic model using both OLS and IV techniques.

I then extend the model to account for the spatial nature of the data in several ways. The most straightforward is s spatial autoregressive model with spatial autoregressive

disturbances (SARAR); see Anselin and Florax (1995) and Drukker, Prucha, and Raciborski (2013b,a) for some background and information on the practical implementation and estimation of the model. This is described in equations 5.2 and 5.3:

$$y_i = \beta_0 + \theta \mathbf{Wy} + \beta_1 SlaveRatio1872_i + \mathbf{x_i}\phi + c_s + u_i \tag{5.2}$$

$$u_i = \rho \mathbf{Mu} + z_i \tag{5.3}$$

where z are idiosyncratic innovations and W and M are non-stochastic spatial weights matrices, with zeros on the main diagonal, which weigh observations based on the inverse of the distance between the centroid of municipality i and all other observations. This means that the spatial lag for observation i will be:

$$\overline{y}_i = \sum_{j=1}^{n} w_{ij} y_j$$

where w_{ij} is the element of matrix $\mathbf{W}$ that identifies the inverse of the distance between observations i and j. This model accounts for spillover effects by explicitly taking into account the effect of neighbors' decisions on the decision of each municipality with regards to the outcome. In this application $W = M$, so the error term is similarly spatially weighted to account for spatial autocorrelation. Once again, I use both OLS and IV techniques to estimate this model.

SARAR type of models have been criticized due to endogeneity issues. If neighbors' outcomes influence the outcome of each unit of observation, including a spatial lag of the outcome in the regression will mechanically introduce a bias since the neighbor's outcome will also be influenced by the outcome of the unit being observed. In order to overcome this issue, Gibbons and Overman (2012) suggest including spatial lags of the explanatory variables, rather than of the outcome. This "neighbor" model is outlined in equation 5.4

$$y_i = \beta_0 + \beta_1 \mathbf{WSlaveRatio1872} + \mathbf{x_i}\phi + \mathbf{WX}\gamma + c_s + u_i \tag{5.4}$$

where **WSlaveRatio1872** and **WX** are the spatial lags of the main explanatory variable and of the controls respectively. I run such a model in my analysis using IV techniques and, in order to further diminish worries of endogeneity, I use the instrumented spatial lag of the slave ratio as the main explanatory variable instead of the municipalities' own slave ratio, in a similar spirit to Acemoglu, García-Jimeno, and Robinson (2015). As a final

exercise, I run a model using three stage least squares (3SLS) in which the spatial lag of the neighbor's outcome is predicted using the spatial lag of the instrumental variables and of the controls and then introduced in an IV estimation of the impact of slavery on the outcome. In this way, I avoid the pitfalls of the SARAR model, but am still able to detect any spillover effects between municipalities, which are lost in the type of model described in equation 5.4.

5.4 Data

In the analysis, I rely on data from three cross sections: 1836, 1872 and 1908-1912. The 1872 data, which I use to construct my slavery measure and most of my controls, comes from Brazil's first country-wide census (Brazil, 1876). The 1836 data used to construct additional controls for São Paulo only are from Bassanezi (1998), who relies on information provided by Müller (1838). The main outcome variables – public revenue and expenditure for 1908-1912 – are from the 1908-12 statistical yearbook (Brazil, 1908-1912) as is the data on population. Additional outcome variables for São Paulo referring to the allocation of public expenditure are from São Paulo's statistical yearbook of 1912 (São Paulo, 1912).

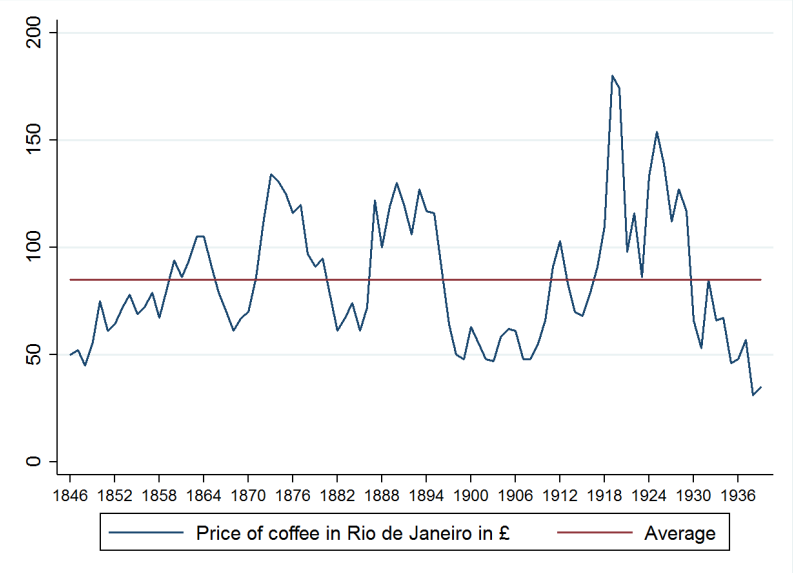


Figure 5.5: Coffee Prices, 1870-1939

Source: Blattman, Hwang, and Williamson (2007)

The choice of the three cross sections is partly motivated by data availability. Fortunately, however, these dates also capture three fundamental periods in Brazilian history. The 1836 cross section offers information on the early phase of the coffee boom and of the development of São Paulo as a major economic and slave centre. The 1872 data capture

the peak of slave numbers in Southeastern Brazil, as well as the initial phase of the deep structural changes to hit the country in the second half of the 19th century.

The 1908-12 endpoint is ideal for my analysis for several reasons. First, it is temporally far enough from the abolition of slavery in 1888 to rule out a direct effect on the outcomes. At the same time, it is not too far removed to plausibly argue for the existence of a persistent indirect effect of slavery. Second, it lies around the middle of Brazil's Old Republic (1889-1939). This was an era of relative decentralization, which means that local governments were important actors within the public sector. The early 20th century, in particular, was characterized by marked improvements and expansions in municipal public services in São Paulo (Rowe, 1908) making an analysis of this period particularly meaningful. Third, the cross-section captures the final stage of pre-WWI mass migration from Europe to Brazil. Given the importance of migration in my story, it is crucial that my period of observation encompasses this migratory flow. Fourth, 1908-12 is a convenient endpoint to allow for a sufficient number of colonies to have been built after my period of observation and to be used for placebo tests. Finally, coffee prices, which presumably had an important impact on public revenues and could thus influence my results, were close to their long-run average in this period (Figure 5.5).

Below, I discuss my outcome variables, data on slavery and the geographical controls. Appendix 5.B provides a summary of the sources and of the methods used to construct all the variables in the analysis.

5.4.1 Data on slavery and public finance

Figure 5.6 illustrates my main variable of interest: the incidence of slaves in the total population in 1872 . The borders of the municipalities are those of 1911-12. As with all the maps I use, these are based on infortmation provided by the Instituto Brasileiro de Geografia e Estatística (IBGE, 2011). The variation across municipalities is very large: slaves made up from around 1% of the population to over 60%. As expected, Rio de Janeiro had many more slaves than São Paulo in relative and absolute terms. The eastern and north-central regions of São Paulo, however, also had large slave populations. The former was home to the initial phases of the coffee boom, while the latter experienced its directly subsequent expansion. The southern and western parts of the state had a much lower incidence of slave labour.

The main outcome variables of the analysis – revenue and expenditure per capita in 1908-12 (Figures 5.7 and 5.8) – also offer a vast degree of variation, especially within São

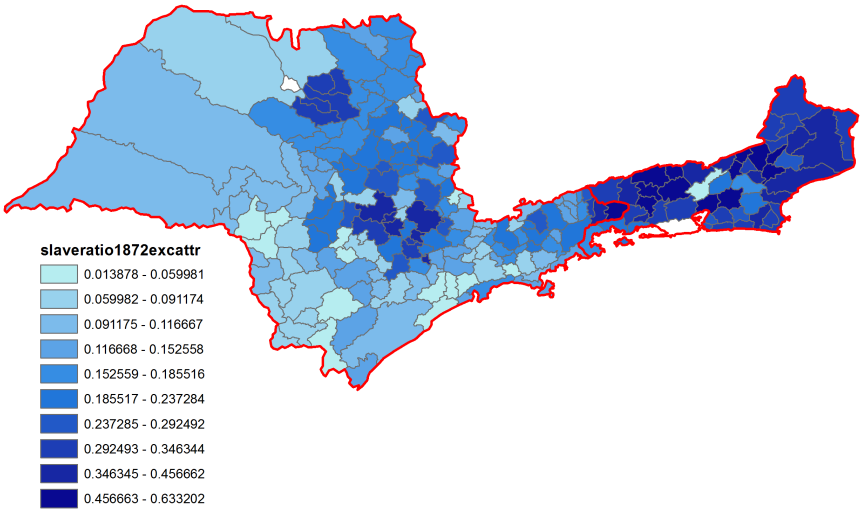


Figure 5.6: Slave ratio in Rio de Janeiro and São Paulo, 1872

Author's calculation. Source: Brazil (1876)

Paulo. Municipal intakes and outlays ranged between less than 0.2 milreís per capita and more than 150. São Paulo emerges as substantially more fiscally endowed than Rio de Janeiro, reflecting the emergence of the province as Brazil's economic powerhouse.[82] In line with the historical narrative, the city of São Paulo, Santos – São Paulo's main port – and the north-central region – a major coffee-producing area – emerge as the most fiscally endowed areas in the province.

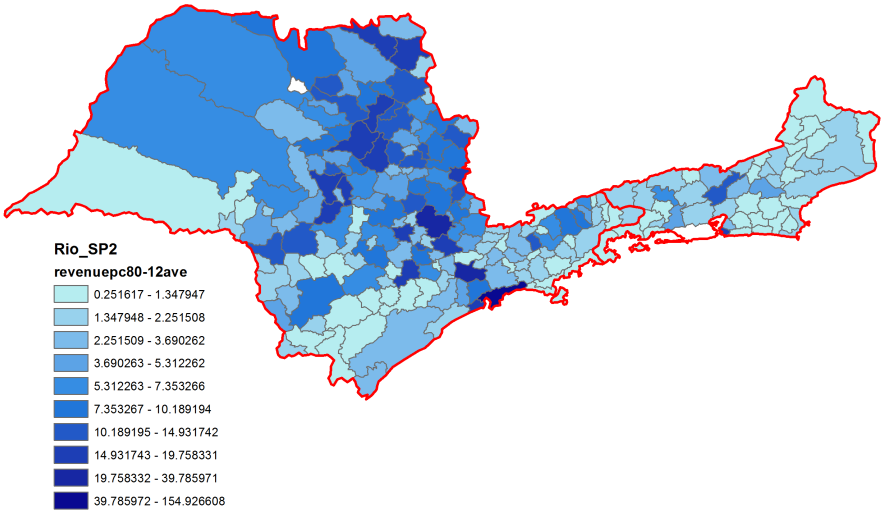


Figure 5.7: Municipal public revenue per capita in in Rio de Janeiro and São Paulo, 1908-12

Author's calculation. Source: Brazil (1908-1912)

[82] 1908-1912 also saw a temporary increase in municipal revenue collection in São Paulo. In the analysis below, I exploit the variation within the two states, to eliminate potential confounding effects due to this.

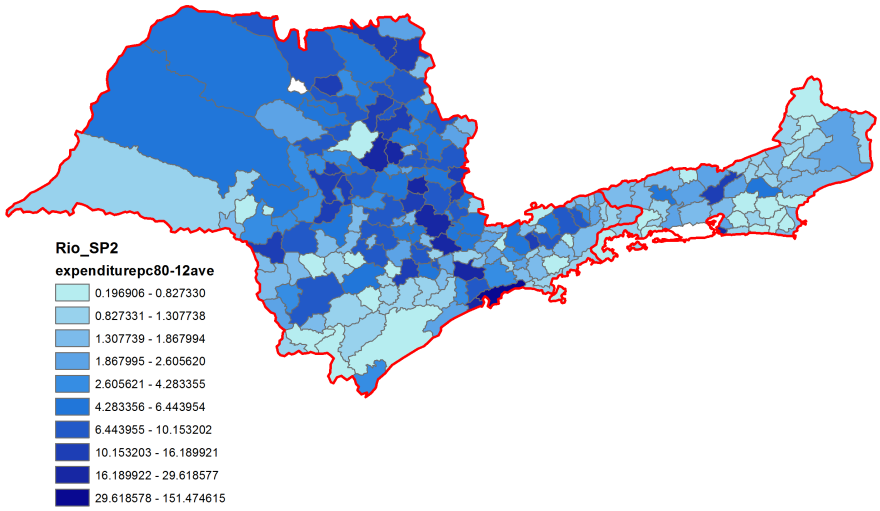


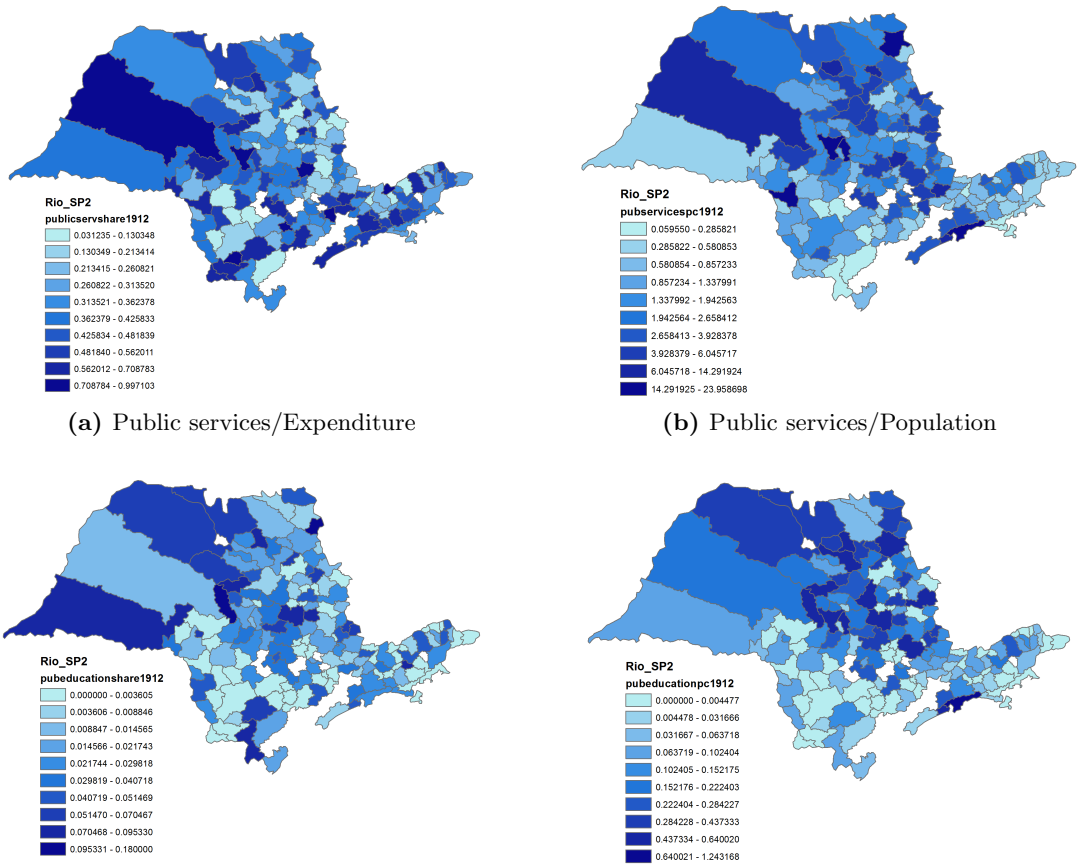
Figure 5.8: Municipal public expenditure per capita in Rio de Janeiro and São Paulo, 1908-12

Author's calculation. Source: Brazil (1908-1912)

Figure 5.9 offers an illustration of the additional outcomes variables available for São Paulo in 1912. These are: 1) municipal expenditure dedicated to public services – public works, street cleaning, public lighting, public healthcare, the running costs of markets, cemeteries and slaughterhouses, public water, public sewers and municipal education – as a share of total public expenditure; 2) municipal per capita expenditure on these same public services; 3) municipal expenditure dedicated to education as a share of total expenditure; 4) municipal per capita expenditure on education.

These variables offer a much broader perspective on local institutions and, consequently, on the impact of slavery on developmental outcomes. The variables provide information not only on the fiscal resources commanded by municipalities, but also on what share of these was dedicated to the provision of essential public goods and other public services. Moreover, they provide genuinely additional information regarding local institutions because they are only imperfectly correlated with each other and with revenue and expenditure per capita. The shares of expenditure dedicated to public services and education, for example, exhibit a very low and statistically insignificant correlation with revenue and expenditure per capita. The per capita public services and education variables are unsurprisingly significantly correlated with total expenditure per capita. However, I show below that slavery had an impact on these outcomes even after controlling for total expenditure per capita.

Data on slave numbers in 1836 for São Paulo allows a comparison of the incidence of slavery over time (Figure 5.10). Although 1872 likely represented the peak year of



(a) Public services/Expenditure

(b) Public services/Population

(c) Public education/Expenditure

(d) Public education/Population

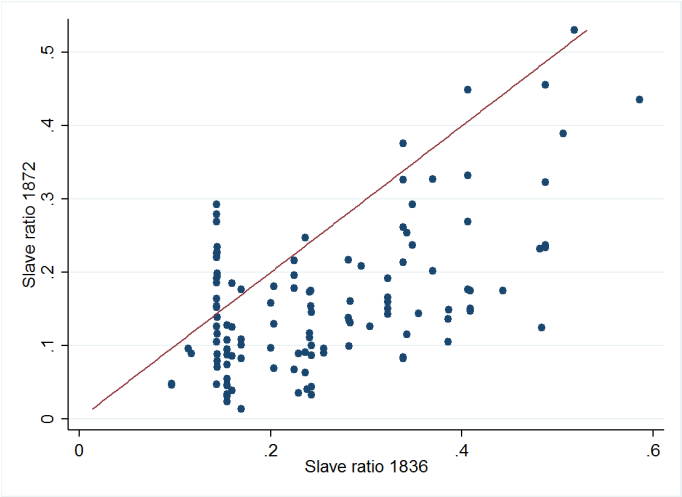
Figure 5.9: Public goods provision in São Paulo, 1912

Author's calculation. Source: São Paulo (1912)

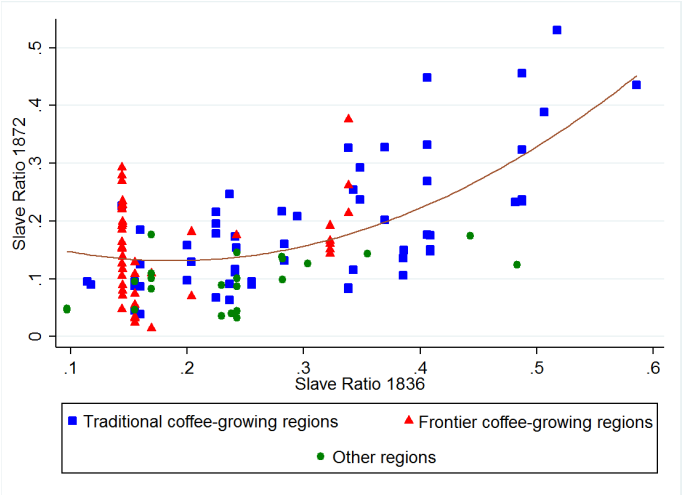
slavery in São Paulo in absolute numbers, in relative terms this was not the case. In fact, slave ratios in 1872 tended to be smaller than at the earlier date. Nonetheless, some municipalities had relatively more slaves in 1872 than in 1836. In both 1836 and 1872, slavery was particularly prevalent in the traditional coffee growing areas of the Paraíba valley and the Central region, confirming that these areas continued to rely heavily on slave labor in the second half of the 19th century. The pace at which the incidence of slave labour changed between 1836 and 1872, however, was very uneven across municipalities, offering a large degree of variation to exploit in my analysis.

5.4.2 Geographical controls and land suitability

In order to account for geographical factors in my analysis, I produce the following municipal level controls: altitude, latitude, longitude, area and distance from the principal port (Rio de Janeiro or Santos, whichever is closer to the centroid of each municipality). I furthermore calculate municipal land suitability indicators based data from the Food and



(a) Slave ratios in 1836 and 1872 and 45°line



(b) Slave ratios in 1836 and 1872, by region

Figure 5.10: Slavery in São Paulo: 1836 vs 1872

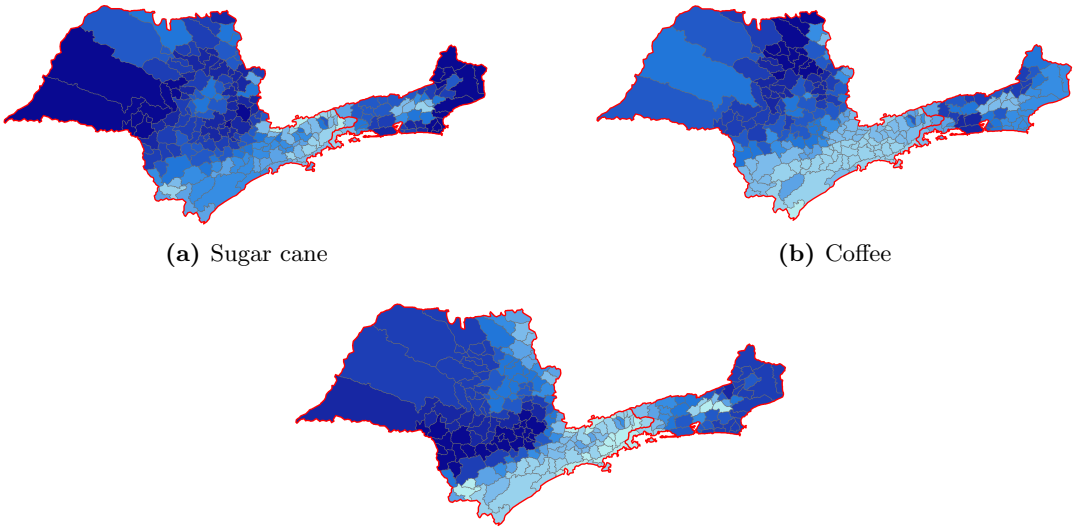
Author's calculation. Traditional coffee-growing regions are: Vale do Paraíba and Central. The frontier areas are: Alta Sorocaba, Noroeste, Araraquarense, Baixa Paulista, and Mogiana. Source: Bassanezi (1998), Müller (1838).

Agriculture Organization (2012), which exploits monthly statistics on climatic variables and precipitation.

Although the FAO land suitability data is constructed using data from the 1960s until the 1990s, it is also extremely useful and widely used in historical studies. To get as close as possible to conditions faced by planters in the 19th and early 20th century, I use suitability data based on the absence of irrigation and the lowest possible level of inputs by planters. The low input scenario is essentially a subsistence agriculture situation, with labor intensive techniques, no use of chemicals and minimal conservation measures. These assumptions are conservative given that the production of cash crops in São Paulo and Rio de Janeiro was well beyond subsistence and that, in the second half of the 19th century,

mechanization of some processes had started to take place.

Figure 5.11 illustrates the suitability of land for the production of the three key crops I use in my analysis: sugar, cotton and coffee. The data squares nicely with historical accounts of crops cultivation. The Paraíba valley, located across the border between the two states, stands out as a relatively suitable area for coffee production. Indeed, it was the first large-scale coffee production centre in the country. The north and west of São Paulo also emerge as very suitable for coffee production and, in fact, these areas witnessed a huge expansion in coffee production in the late 19th and early 20th century, as virgin and semi-virgin lands were cleared of forest and exploited for agricultural production.



(a) Sugar cane

(b) Coffee

(c) Cotton

Figure 5.11: Land suitability for cash crops production in Rio de Janeiro and São Paulo

Author's calculation. Source: Food and Agriculture Organization (2012). A darker shade of blue indicates higher suitability for the production of each crop.

Regarding sugar cane, the east of Rio De Janeiro emerges as a particularly suitable area for its production, as was indeed historically. The west and north of São Paulo also appear to be suitable for this cultivation, as well as for that cotton. However, the late exploitation of the province's backlands means that São Paulo was never a big player in the production of these two commodities, although sugar was the province's main export before the arrival of coffee. Although the land suitability data is clearly spatially clustered, it still offers a large degree of variation even between neighboring municipalities, which I exploit in my analysis.

5.5 Results

I organize my results in five parts. Section 5.5.1 presents the basic OLS analysis for São Paulo. Section 5.5.2 illustrates the results of the standard IV analysis, while section 5.5.3 outlines the results of the spatial regressions. Section 5.5.4 explores the channels through which slavery affected fiscal development, by focusing on the immigrants settlement channel. Finally, Section 5.5.5 adds Rio de Janeiro to the analysis.

5.5.1 Basic analysis for São Paulo

I start my analysis for São Paulo by relating the incidence of slavery in 1872 to my six outcome variables, including only basic controls: a dummy for Santos – which is a clear outlier – latitude, longitude, altitude, distance from the closest port and the suitability of land for the production of coffee, sugar and cotton, and using simple ordinary least squares (Table 5.1).[83] Based on these results, one could conclude that the incidence of slavery positively influenced the development of fiscal capacity, as suggested by the positive coefficients on the slavery indicator for revenue and expenditure per capita. For the other outcomes variables, the results are inconclusive. However, as argued above, these estimates are marred by multiple sources of bias and are no more than conditional correlations. They simply indicate that municipalities with relatively more slaves in 1872, tended to have higher revenue and expenditure per capita in 1908-12

I continue the analysis in Table 5.2 by adding all my controls variables for both 1836 and 1872. The inclusion of the 1872 variables is essential in order to isolate the effect of slavery from that of competing factors. The 1836 controls are also important, as they provide information about earlier differences across São Paulo's regions, which might have affected both the successive incidence of slave labour and the development of local fiscal institutions.

The 1836 controls are: 1) the incidence of slavery both as a standard variable and as a quadratic term, to account for local conditions which made some regions particularly amenable to slave labour; 2) the size of the population, to account for differences between small and large urban centers; 3) the number of tax collectors per capita, as a proxy for historical state oversight and fiscal capacity; 4) sugar and coffee production per capita, to account for the early presence of these profitable activities intimately linked to slave labour.

[83] Altitude is dropped from successive regressions to preserve degrees of freedom, as it is insignificant in all specifications.

Table 5.1: Basic OLS estimation

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	RevPerCap	ExpPerCap	PubServShare	PubServPerCap	PubEduShare	PubEduPerCap
SlaveRatio1872	12.76**	12.22*	-0.278	0.160	-0.00747	0.226
	(6.355)	(6.328)	(0.170)	(3.352)	(0.0272)	(0.179)
Constant	-4.560	-2.795	-1.803	-4.227	0.173	0.475
	(27.93)	(23.23)	(1.198)	(12.93)	(0.167)	(0.705)
Additional controls						
Santos dummy	✓	✓	✓	✓	✓	✓
Geo variables	✓	✓	✓	✓	✓	✓
Land suitability	✓	✓	✓	✓	✓	✓
Observations	173	173	161	161	161	161
R-squared	0.834	0.864	0.089	0.245	0.129	0.464

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Revenue and expenditure per capita are 1908-1912 averages, the other outcome variables are measured in 1912. Geographic controls include: longitude, latitude, altitude, distance from the port of Santos or Rio de Janeiro (whichever is closer to the centroid of the municipality). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. See Appendix 5.B for information on how the controls are constructed.

The 1872 controls are: 1) the number of public employees per capita, which proxies the 1872 level of fiscal and state capacity; 2) the share of citizens who could read and write, to account for differences in human capital, prosperity and educational provision; 3) the share of citizens born outside the state living in the municipality, to control for internal migration; 4) the share of citizens born outside of Brazil (excluding slaves) living in the municipality, to account for foreign immigration (both migration variables are also meant to control for the prosperity and growth prospects of the local economy); 5) the share of citizens working in agriculture, to control for the rural focus of the municipalities; 6) the size of the population both as a standard variable and as quadratic, to account for differences between municipalities containing large and smaller urban centers and connected non-linearities; 7) population density, a commonly used proxy of prosperity and urbanization.

In addition, I control for the degree of ethnic fractionalization using the index proposed by Alesina, Baqir, and Easterly (1999) and for the share of non-whites in the population, both measured in 1872. I use the share of non-whites alongside the more sophisticated fractionalization index because the latter might not capture racial cleavages well in the Brazilian context, which was characterized by a relatively porous concept of race. These variables are particularly important for three reasons. First, the ethnic make up of the municipalities is clearly and directly related to the incidence of slavery. Second, previous

Table 5.2: OLS estimation with additional controls

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	RevPerCap	ExpPerCap	PubServShare	PubServPerCap	PubEduShare	PubEduPerCap
SlaveRatio1872	7.722	7.750	-0.411	-7.519	-0.00186	-0.0596
	(7.333)	(6.106)	(0.320)	(4.696)	(0.0496)	(0.226)
Constant	34.22	43.02	-3.212*	9.347	-0.0522	0.789
	(46.80)	(33.29)	(1.710)	(20.73)	(0.222)	(1.085)
Additional controls						
Geo variables	✓	✓	✓	✓	✓	✓
Land suitability	✓	✓	✓	✓	✓	✓
1836 variables						
SlaveRatio	✓	✓	✓	✓	✓	✓
SlaveRatioSq	✓	✓	✓	✓	✓	✓
PopSize	✓	✓	✓	✓	✓	✓
TaxCollector	✓	✓	✓	✓	✓	✓
SugarProd	✓	✓	✓	✓	✓	✓
CoffeeProd	✓	✓	✓	✓	✓	✓
1872 variables						
StateCapacity	✓	✓	✓	✓	✓	✓
Literacy	✓	✓	✓	✓	✓	✓
NewMunicipality	✓	✓	✓	✓	✓	✓
DomMigrants	✓	✓	✓	✓	✓	✓
ForMigrants	✓	✓	✓	✓	✓	✓
ShareAgricWork	✓	✓	✓	✓	✓	✓
PopSize	✓	✓	✓	✓	✓	✓
PopSizeSq	✓	✓	✓	✓	✓	✓
PopDensity	✓	✓	✓	✓	✓	✓
NonWhiteShare	✓	✓	✓	✓	✓	✓
EthniFrac	✓	✓	✓	✓	✓	✓
Observations	171	171	159	159	159	159
R-squared	0.898	0.914	0.144	0.341	0.158	0.547
Robust standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Revenue and expenditure per capita are 1908-1912 averages, the other outcome variables are measured in 1912. Geographic controls include: longitude, latitude, distance from the port of Santos or Rio de Janeiro (whichever is closer). Altitude is insignificant and thus excluded to preserve degrees of freedom. The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. See Appendix 5.B for information on how the controls are constructed.

work suggests a negative relationship between ethnic diversity and public goods provision (Alesina, Baqir, and Easterly, 1999; Alesina and La Ferrara, 2005),[84] which means accounting for this factor is essential in order to disentangle its effect from that of slavery. Third, if economic outcomes such as wages and income differed between whites, blacks, mulattos and mestizos, geographical variation in fiscal capacity might simply reflect differences in the local ethnic composition, rather than the institutional characteristics of municipalities.

[84] A similar relationship has been proposed by Lieberman (2003) for Brazil at the national level, but it might have hampered the development of fiscal capacity at a more micro level as well.

I have also experimented with further controls, such as the share of foreigners amongst the economic elite, the ethnic polarization index proposed by Reynal-Querol (2002)[85] and the local income per capita estimates for 1872 produced by Monasteiro (2010).[86] These yielded insignificant coefficients and did not change the other results. Therefore, I have excluded them from the analysis to preserve degrees of freedom.[87]

The introduction of all the controls eliminates the statistically significant positive association between slavery and fiscal capacity found in the previous exercise. Nonetheless, the coefficients remain positive for revenue and expenditure per capita. For the other outcome variables, they are negative but statistically insignificant. As discussed above, in order to draw inference on the relationship between slavery and the fiscal outcomes, it is insufficient to include even such a wide array of controls, as some unobservable factors might still be driving both the incidence of slavery and the outcomes. Moreover, the spatial nature of the data is not yet accounted for. In order to move towards causal inference, I now turn to the IV analysis.

5.5.2 IV analysis for São Paulo

My IV strategy is outlined in detail in Section 5.3.1. Table 5.3 illustrates the first stage of the regression for the whole sample and for the restricted sample for which the public goods data are available. The first stage F-statistics are all safely above the commonly used threshold of 10, which indicates a sufficiently strong instrument.

As discussed above, the instrument – the log of the interaction between land suitability for coffee production and distance from the closest port – is meant to capture how the timing of exploitation of land for coffee production affected the reliance of municipalities on slave labour. The first stage of the IV indicates that as land suitability for coffee production increased, distance to the coast diminished in importance as a determinant for the incidence of slavery. On the flip side, this means that land suitability decreased in importance as a determinant of the slave ratio as municipalities got closer the port. Consistently with the historiography, I interpret this to reflect the fact that areas closer to the port were settled earlier, while both the domestic and the international slave trade were still thriving. Coffee suitability played a small, if any, role in determining the slave ratio in this slave-abundant setting. Frontier areas, instead, were settled as slavery was

[85] See also Montalvo and Reynal-Querol (2005).

[86] Monasteiro's estimates refer to minimum comparable areas, rather than municipalities, so this test was performed by aggregating municipalities to form these geographical units.

[87] Some of these variables are also highly correlated with the other controls, leading to multicollinearity and less precision in the estimates.

Table 5.3: First stage of the IV

	(1)	(2)
	Full sample	Restricted sample
VARIABLES	SlaveRatio1872	SlaveRatio1872
LN(CoffeeSuit*DistPort)	0.0586***	0.0650***
	(0.0177)	(0.0197)
DistPort	-0.000612***	-0.000657***
	(0.000116)	(0.000131)
CoffeeSuit	-0.000110	-0.000144
	(9.47e-05)	(0.000100)
SlaveRatio1836	-0.650**	-0.555*
	(0.263)	(0.289)
SlaveRatio1836sq	1.743***	1.546***
	(0.468)	(0.516)
Literacy1872	-0.187***	-0.217***
	(0.0479)	(0.0521)
ForMigrants1872	0.843***	0.801**
	(0.318)	(0.333)
ShareAgricWork1872	0.0991***	0.113***
	(0.0345)	(0.0400)
PopSize1872	7.26e-06**	7.38e-06**
	(2.87e-06)	(3.15e-06)
PopSize1872sq	-2.34e-10**	-2.34e-10**
	(9.74e-11)	(1.07e-10)
EthniFrac1872	0.246***	0.301***
	(0.0643)	(0.0761)
Constant	0.596**	0.645**
	(0.278)	(0.272)
Additional controls		
Other 1836 variables	✓	✓
Other 1872 variables	✓	✓
Geo variables	✓	✓
Land suitability	✓	✓
Observations	171	159
R-squared	0.696	0.699
Robust standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

The restricted sample refers to the municipalities for which the additional public goods outcome variables are available. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Santos is excluded from the estimation. I drop the non white share variable and the coffee and sugar production variables in order to preserve degrees of freedom. They are insignificant and do not change the results qualitatively or quantitatively. I also drop longitude because, while not statistically significant, it is closely related to my instrument, and decreases its strength when included. All other controls included in previous estimations remain.

in sharp decline or already abolished and European migration was taking off. Therefore, these municipalities could not generally rely on large scale forced labour, but could count on immigration as an increasingly reliable substitute. In this slave-scarce setting, coffee suitability played an important role, and only very fertile areas with expanding coffee plantations were able to attract relatively large numbers of slaves.

The incidence of slavery is strongly related to the majority of the other controls included in the regression. Unsurprisingly, municipalities with more slaves were characterized by lower literacy, a higher share of workers in agriculture and more ethnic fraction-

alization. Some more unexpected and interesting relationships are also evident. First, municipalities with more slaves also tended to have more foreign-born citizens. Importantly, this result does not in any way invalidate my argument that slavery discouraged the settlements of foreign immigrants. The results of this first stage are simple conditional correlations, which most likely reflect the fact that booming areas attracted large numbers of both free and coerced workers. As such, it is a strong suggestion that the migration variables capture at least part of the prosperity and growth potential of the municipalities. Second, up to a certain point more populous municipalities tended to have more slaves, as suggested by the positive coefficient on the population size variable. However the largest urban centers, captured by the quadratic population size term, tended to have relatively less slaves. Finally, the relationship between slavery in 1836 and 1872 exhibits a U shape. For an incidence of slavery of up to around 20%, regions with more slaves in 1836 tended to have relatively less slaves in 1872. However, large slave centers in 1836, identified by the quadratic slave ratio term, also had large numbers of slaves in 1872, confirming the persistence of the institution in traditional slave-based economies.

Table 5.4 illustrates the results of the second stage of the IV estimation. What emerges clearly is that slavery had a negative effect on the development of fiscal capacity and public goods provision. This effect is evident on revenue per capita, expenditure per capita, the public services share, public services expenditure per capita and education expenditure per capita and is statistically significant and large. I find no effect of slavery on the share of expenditure dedicated to education, but I recover this result in the spatial analysis.

The estimates indicate that a 10 percentage points increase in the slave ratio (which is slightly more than a one standard deviation increase), would have reduced revenue per capita by around 8.4 milreís and expenditure per capita by around 6.7 milreís . Expenditure on public services and education would have been lower by around 4 and 0.2 milreís per capita respectively. These are very large effects given that average revenue and expenditure per capita, excluding Santos, were around 7 and 6 milreís per capita respectively. Expenditure on public services and public education was approximately 2.5 and 0.1 milreís respectively. In the spatial analysis below, I find smaller, more realistic coefficients. This suggests that spatial techniques can take care of additional bias not accounted for by the IV strategy alone. I now turn to illustrating the results of these estimations.

Table 5.4: Standard IV estimation

Panel A: Second Stage						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	RevPerCap	ExpPerCap	PubServShare	PubServPerCap	PubEduShare	PubEduPerCap
SlaveRatio1872	-84.16**	-67.31**	-2.395**	-41.522**	-0.116	-2.307**
	(41.43)	(32.63)	(1.214)	(20.478)	(0.173)	(1.120)
Constant	125.2***	121.9***	2.027	74.612**	0.348*	4.950***
	(44.09)	(39.29)	(1.883)	(28.355)	(0.192)	(1.328)
Additional controls						
Geo variables	✓	✓	✓	✓	✓	✓
Land suitability	✓	✓	✓	✓	✓	✓
1836 variables	✓	✓	✓	✓	✓	✓
1872 variables	✓	✓	✓	✓	✓	✓
Observations	171	171	159	159	159	159
R-squared	-0.014	0.040	-0.148	0.0284	0.128	-0.015
Panel B: First Stage Statistics						
F-stat	11.00***	11.00***	10.91***	10.91***	10.91***	10.91***
Part R-squared	0.0457	0.0457	0.0548	0.0548	0.0548	0.0548
Robust standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Revenue and expenditure per capita are 1908-1912 averages, the other outcome variables are measured in 1912. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Santos is excluded from the estimation. I drop the non white share variable and the coffee and sugar production variables in order to preserve degrees of freedom. They are insignificant and do not change the results qualitatively or quantitatively. I also drop longitude because, while not statistically significant, it is closely related to my instrument, and decreases its strength when included. All other controls included in previous estimations remain. Standard errors are heteroskedaticity robust, but results do not change if I cluster them at the 1872 municipal level.

5.5.3 Spatial analysis for São Paulo

As discussed above, if ignored, the spatial nature of the data can lead to bias and incorrect standard errors, which would affect the reliability of the results . In order to account for this, I run my spatial analysis in the three steps discussed in Section 5.3.2. The first step is the SARAR IV estimation, which accounts for both spillovers between municipalities and spatial autocorrelation in the error term. The second step is the "neighbor" IV estimation suggested by Gibbons and Overman (2012), to account for the potential bias of the SARAR model. I illustrate the results of this exercise in Appendix 5.A in order to reduce the number of regression tables in the text. These are broadly qualitatively consistent with the SARAR IV results. The final step in the analysis is a 3SLS approach, which aims to avoid the pitfalls of the SARAR model, like the "neighbor" approach, but which also estimates spillover effects between municipalities directly, unlike the latter.

I illustrate the results of SARAR IV estimation in Table 5.5. Compared to the standard IV regressions, I also control for overall expenditure per capita when estimating the impact of slavery on expenditure per capita on public services and on education (columns 4 and 6), but the results are robust to excluding the variable. I do this to further demonstrate that slavery did not only reduce overall public resources, but also affected their allocation. The coefficients in the Table are substantially smaller than those of the standard IV estimation. A 10% increase in the slave ratio decreased revenue and expenditure per capita by 2 and 1.4 milreís per capita respectively. The effect for public service expenditure is 0.8 and 0.06 milreís per capita respectively. The overall message, however, is unchanged: slavery had a strong negative effect on fiscal development and public goods provision in São Paulo. The only exception is due to the slavery coefficient for the public services share becoming statistically insignificant. However, I recover the statistically significantly negative result using the more sophisticated technique of step three. In line with the findings of Acemoglu, García-Jimeno, and Robinson (2015) for Colombia, positive and statistically significant coefficients for lambda – which represents the spatial lag of the dependent variable – suggest positive spillover effects from neighboring municipalities in expenditure per capita and public education expenditure per capita.

I present the results of the final step of my spatial analysis in three tables: Table 5.6 for revenue and expenditure per capita, Table 5.7 for public services, and Table 5.8 for education. The main results of the estimations are in columns 1 and 5 of each table. These illustrate the effect of the slavery indicator and of the spatial lag of the outcome on the outcome itself. The slave ratio used in the estimation is predicted in columns 2 and and 6 of each Table using the same instrument and controls as in the standard IV above. The spatial lag of the outcome is predicted in columns 3 and 7 using the spatial lag of the controls and of the slave ratio, which, in turn, is predicted in columns 4 and 8 using the spatial lag of the instrument. All four equations for each outcome variable are treated as a single system of equations, with the error term correlated across all equations.

The results of the estimation are quantitatively somewhere in between the standard and SARAR IV estimates. Qualitatively, the negative impact of slavery on all six outcomes emerges clearly and strongly. The positive and statistically significant coefficients of the spatial lag of the outcome for total expenditure and expenditure on public education further support the existence of positive spillover effects and strategic complementarities in fiscal capacity and public goods provision across municipalities.

For further robustness, I provide an additional important test in Appendix 5.A. This

Table 5.5: SARAR IV estimation

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	RevPerCap	ExpPerCap	PubServShare	PubServPerCap	PubEduShare	PubEduPerCap
SlaveRatio1872	-20.38***	-14.13**	-0.326	-8.042**	-0.0652	-0.592**
	(7.292)	(6.864)	(0.366)	(3.786)	(0.0498)	(0.243)
ExpPerCap				0.383***		0.0137***
				(0.100)		(0.00253)
Constant	41.67	38.29	-0.101	2.418	0.155	1.330*
	(32.58)	(26.29)	(1.262)	(15.55)	(0.146)	(0.728)
Lambda	0.500	0.723*	-0.190	0.382	1.016	0.832*
	(0.377)	(0.392)	(0.397)	(0.720)	(0.683)	(0.470)
Rho	-1.324	-0.835	-0.00194	-1.815	-3.061	-1.662
	(1.255)	(1.754)	(0.936)	(1.877)	(1.959)	(1.565)
Additional controls						
Geo variables	✓	✓	✓	✓	✓	✓
Land suitability	✓	✓	✓	✓	✓	✓
1836 variables	✓	✓	✓	✓	✓	✓
1872 variables	✓	✓	✓	✓	✓	✓
Santos dummy	✓	✓	✓	✓	✓	✓
Observations	171	171	159	159	159	159
R-squared	-0.014	0.040	-0.148	0.0284	0.128	-0.015
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Revenue and expenditure per capita are 1908-1912 averages, the other outcome variables are measured in 1912. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Santos is excluded from the estimation. I drop the non white share variable and the coffee and sugar production variables in order to preserve degrees of freedom. They are insignificant and do not change the results qualitatively or quantitatively. I also drop longitude because, while not statistically significant, it is closely related to my instrument, and decreases its strength when included. All other controls included in previous estimations remain. Lambda and rho are the coefficients of the spatial lag of the dependent variable and of error term respectively.

demonstrates that my results do not change if I exclude two potentially problematic revenue items from my revenue per capita measure: loans and government subsidies. In the Appendix, I explain the results of this exercise in detail.

5.5.4 The settlement channel

In Section 5.3, I discussed the various channels through which slavery might have affected fiscal capacity and public goods provision. Empirically, I focus on one key mechanism: the immigrant settlement channel described in detail in the same section. In brief, the hypothesis is that the settlement of migrants stimulated the development of fiscal capacity and public goods provision in the province, and that settlement patterns were, in turn, at least partially determined by the incidence of slavery.

Table 5.6: Revenue and expenditure, 3SLS estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	RevPerCap	SlaveRatio1872	SL_RevPerCap	SL_SlaveRatio1872	ExpPerCap	SlaveRatio1872	SL_ExpPerCap	SL_SlaveRatio1872
SL_RevPerCap	0.638				0.915**			
	(0.405)				(0.400)			
SlaveRatio1872	-33.35**				-21.02*			
	(13.07)				(11.67)			
CoffeeSuit	0.0160**	-0.000107			0.00878	-0.000103		
	(0.00732)	(9.80e-05)			(0.00658)	(9.84e-05)		
DistPort	0.00421	-0.000582***			0.00815	-0.000579***		
	(0.0143)	(0.000131)			(0.0126)	(0.000131)		
LN(CoffeeSuit*DistPort)		0.0568***				0.0561***		
		(0.0200)				(0.0201)		
SL_SlaveRatio1872			-97.44***				-78.55***	
			(12.12)				(11.48)	
SL_CoffeeSuit			0.0587***	-0.000355***			0.0575***	-0.000357***
			(0.00827)	(8.30e-05)			(0.00782)	(8.35e-05)
SpatLagDistPort			-0.0340***	-0.000779***			-0.0380***	-0.000796***
			(0.00880)	(9.93e-05)			(0.00832)	(9.99e-05)
SL_LN(CoffeeSuit*DistPort)				0.110***				0.111***
				(0.0114)				(0.0115)
Constant	35.70	0.563**	0.193	0.00383	30.96	0.566**	0.301	0.00410*
	(32.22)	(0.273)	(0.352)	(0.00246)	(28.35)	(0.274)	(0.332)	(0.00247)
Additional controls								
Geo variables	✓	✓			✓	✓		
Land suitability	✓	✓			✓	✓		
1836 variables	✓	✓			✓	✓		
1872 variables	✓	✓			✓	✓		
Santos dummy	✓	✓			✓	✓		
Spatial lag of controls			✓	✓			✓	✓
Observations	172	172	172	172	172	172	172	172
R-squared	0.870	0.692	0.951	0.997	0.897	0.693	0.948	0.997

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

SL stands for spatial lag. Revenue and expenditure per capita are 1908-1912 averages. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Santos is excluded from the estimation. I drop the non white share variable and the coffee and sugar production variables in order to preserve degrees of freedom. They are insignificant and do not change the results qualitatively or quantitatively. I also drop longitude because, while not statistically significant, it is closely related to my instrument, and decreases its strength when included. All other controls included in previous estimations remain. Lambda and rho are the coefficients of the spatial lag of the dependent variable and of error term respectively.

The problem with testing this hypothesis is that, leaving slavery aside for a moment, immigrants did not distribute themselves randomly across São Paulo's municipalities. Presumably, they tended to locate where labour demand and opportunities were more abundant. These same locations tended to acquire large numbers of slaves. In fact, I find that, in 1872, municipalities with relatively more slaves also had larger shares of foreign born non-slave citizens. Thus, a direct analysis of the impact of slavery on migrant settlement and of the latter on fiscal development would be potentially marred by omitted variable bias, the omitted variables being the demand for labour and growth potential of each municipality. I rely, instead, on the location of settler colonies set up by the national/provincial governments in conjunction with private companies and planters between 1829 and 1939 to proxy the settlement of migrants.

Table 5.7: Public services, 3SLS estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	PubServShare	SlaveRatio1872	SL_PubServShare	SL_SlaveRatio1872	PubServPerCap	SlaveRatio1872	SL_PubServPerCap	SL_SlaveRatio1872
SL_PubServShare	-0.0551							
	(0.419)							
SL_PubServPerCap					1.270			
					(0.806)			
SlaveRatio1872	-1.850***				-20.93**			
	(0.507)				(9.401)			
CoffeeSuit	0.000278	-0.000146			-0.00450	-0.000143		
	(0.000320)	(0.000105)			(0.00575)	(0.000105)		
DistPort	-0.000430	-0.000657***			0.00516	-0.000653***		
	(0.000575)	(0.000134)			(0.00883)	(0.000137)		
LN(CoffeeSuit*DistPort)		0.0654***				0.0656***		
		(0.0208)				(0.0213)		
SL_SlaveRatio1872			-1.033***				-23.60***	
			(0.246)				(4.463)	
SL_CoffeeSuit			0.000577***	-0.000320***			0.0213***	-0.000319***
			(0.000164)	(8.69e-05)			(0.00296)	(8.69e-05)
SL_DistPort			-0.000399**	-0.000812***			-0.0109***	-0.000811***
			(0.000174)	(0.000102)			(0.00314)	(0.000102)
SL_LN(CoffeeSuit*DistPort)				0.110***				0.109***
				(0.0122)				(0.0122)
Constant	1.384	0.633**	0.0123*	0.00384	31.91	0.621**	0.214*	0.00385
	(1.229)	(0.281)	(0.00678)	(0.00242)	(22.37)	(0.283)	(0.123)	(0.00242)
Additional controls								
Geo variables	✓	✓			✓	✓		
Land suitability	✓	✓			✓	✓		
1836 variables	✓	✓			✓	✓		
1872 variables	✓	✓			✓	✓		
Santos dummy	✓	✓			✓	✓		
Spatial lag of controls			✓	✓			✓	✓
Observations	160	160	160	160	160	160	160	160
R-squared	-0.029	0.699	0.990	0.997	0.247	0.698	0.945	0.997

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

SL stands for spatial lag. The outcome variables are measured in 1912. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Santos is excluded from the estimation. I drop the non white share variable and the coffee and sugar production variables in order to preserve degrees of freedom. They are insignificant and do not change the results qualitatively or quantitatively. I also drop longitude because, while not statistically significant, it is closely related to my instrument, and decreases its strength when included. All other controls included in previous estimations remain. Lambda and rho are the coefficients of the spatial lag of the dependent variable and of error term respectively.

The intuition for using these is that municipalities with settler colonies were more likely to attract migrants both directly – the colonies were created through government-subsidized arrivals of Europeans – and indirectly – through migrant networks and family re-conjunctions – than municipalities without settler colonies. At the same time, settler colonies created while slavery was not yet abolished were located, on average, in municipalities with relatively smaller slave populations. For colonies created after the abolition of slavery, instead, there is no negative association between their location and the incidence of slavery. If my hypothesis that slavery affected fiscal capacity development and public goods provision by shaping the settlement of foreign migrants is true, one would expect municipalities with settler colonies founded while slavery still existed to be positively associated with these outcomes and to explain away the effect of slavery.

Table 5.8: Municipal education, 3SLS estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	PubEduShare	SlaveRatio1872	SL_PubEduShare	SL_SlaveRatio1872	PubEduPerCap	SlaveRatio1872	SL_PubEduPerCap	SL_SlaveRatio1872
SL_PubEduShare	0.800							
	(0.842)							
SL_PubEduPerCap					1.092**			
					(0.552)			
SlaveRatio1872	-0.209***				-1.369***			
	(0.0748)				(0.370)			
CoffeeSuit	-8.73e-05*	-0.000137			-9.64e-05	-0.000128		
	(4.76e-05)	(0.000105)			(0.000241)	(0.000104)		
DistPort	6.79e-05	-0.000648***			-8.59e-06	-0.000644***		
	(7.02e-05)	(0.000136)			(0.000349)	(0.000134)		
LN(CoffeeSuit*DistPort)		0.0653***				0.0654***		
		(0.0211)				(0.0208)		
SL_SlaveRatio1872			-0.139***				-1.448***	
			(0.0309)				(0.220)	
SL_CoffeeSuit			8.23e-05***	-0.000316***			0.00143***	-0.000311***
			(2.05e-05)	(8.69e-05)			(0.000147)	(8.64e-05)
SL_DistPort			7.89e-06	-0.000808***			-0.000655***	-0.000795***
			(2.17e-05)	(0.000102)			(0.000156)	(0.000101)
SL_LN(CoffeeSuit*DistPort)				0.109***				0.107***
				(0.0122)				(0.0121)
Constant	0.281	0.605**	0.00214**	0.00381	2.425***	0.564**	0.0187***	0.00397
	(0.179)	(0.282)	(0.000854)	(0.00242)	(0.932)	(0.279)	(0.00605)	(0.00242)
Additional controls								
Geo variables	✓	✓			✓	✓		
Land suitability	✓	✓			✓	✓		
1836 variables	✓	✓			✓	✓		
1872 variables	✓	✓			✓	✓		
Santos dummy	✓	✓			✓	✓		
Spatial lag of controls			✓	✓			✓	✓
Observations	160	160	160	160	160	160	160	160
R-squared	-0.004	0.698	0.957	0.997	0.411	0.697	0.969	0.997
Standard errors in parentheses								
*** p<0.01, ** p<0.05, * p<0.1								

SL stands for spatial lag. The outcome variables are measured in 1912. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Santos is excluded from the estimation. I drop the non white share variable and the coffee and sugar production variables in order to preserve degrees of freedom. They are insignificant and do not change the results qualitatively or quantitatively. I also drop longitude because, while not statistically significant, it is closely related to my instrument, and decreases its strength when included. All other controls included in previous estimations remain. Lambda and rho are the coefficients of the spatial lag of the dependent variable and of error term respectively.

This strategy is potentially invalidated by the fact that settler colonies were not created randomly. Although it does not appear that colonies were located in particularly favorable locations (Cameron, 1931), one could envisage that they were placed in municipalities with more growth potential or better initial institutions. In order to account for this issue, I include colonies created after the end of slavery in my estimation as a separate variable. If settler colonies were indeed placed in more favorable locations and/or if the settlement of immigrants did indeed improve local institutions, these colonies should also be positively associated with my outcomes. In either case, they should not explain away the effect of slavery if my identification strategy is valid. Finally, I also introduce placebo colonies. These are settlements founded after the end of my period of observation, i.e. post-1912. Finding a positive effect of these colonies on my outcomes would be a strong signal that

colonies were simply placed in more favorable locations, and that my identification strategy is not valid. Reassuringly, this is not the case.

The colonies I use in the estimation are detailed in Figure 5.12. The final year for my first group of colonies is 1889 rather than 1888 (the year slavery was abolished) because colonies completed in that year were devised before the abolition of slavery by the Imperial government. The second group of colonies includes those founded between 1890 and 1912, while last group includes colonies founded between 1913 and 1938.[88]

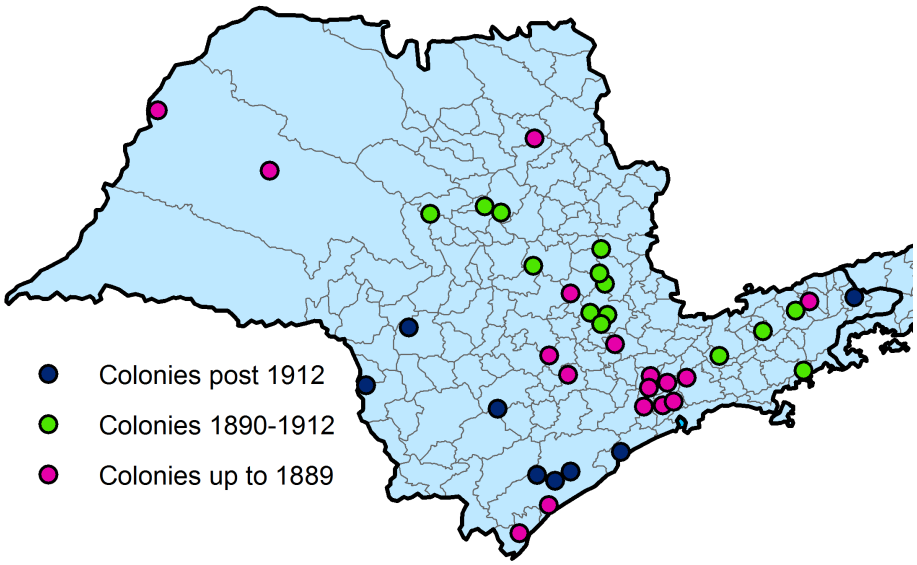


Figure 5.12: The location of settler colonies in São Paulo, 1829-1938

Source: Gagliardi (1958)

Table 5.9 illustrates the results of the exercise using standard IV estimations, but spatial models yield similar results (see Appendix 5.A). The results confirm both the hypothesis and validity of the identification strategy. For expenditure per capita and revenue per capita, the coefficient of the pre-1890 colonies is positive, large and highly statistically significant. Moreover, the inclusion of this variable greatly reduces the size of the slavery coefficient, making it statistically insignificant (columns 1 and 3). When the pre-1890 colonies are excluded, instead, the slavery coefficient retuns to be large and statistically significantly negative, even if the 1890-1912 and post-1912 colonies are included (columns

[88]The creation of first group of colonies was overseen by the imperial government, the second by the provincial government, while in the last group all except two, which were implemented by the Federal government, were implemented by the provincial government, as well. There is no evidence of a discontinuity in the strategy regarding the creation of these colonies between the three periods. If anything, colonies set-up by the provincial government were considered more successful due to improvements in the design of settler contracts and of conditions on plantations. However, the overall experience with this type of immigration was not generally considered to have been particularly successful (Cameron, 1931).

2 and 4). Furthermore, the coefficient of the 1890-1912 colonies is positive and statistically significant, as expected, while the coefficient of the post-1912 placebo colonies is indistinguishable from zero, as required by the identification strategy.

The inclusion of the pre-1890 colonies also reduces the first-stage F-stat indicating that this variable captures part of the variation in the distribution of slaves across the province attributable to the instrument. This suggests that, indeed, forced and free labour stood in contrast to each other while slavery still existed. The inclusion of the post-slavery colonies, instead, has no impact on the first stage statistics. Quantitatively, the impact of the colonies is large: a municipality with a pre-1890 colony had on average nearly 6 milreís higher per capita revenue and approximately 4 milreís higher per capita expenditure. This is in the same ballpark as the effect of a 10 percentage points increase in the slave ratio found above.

For the other outcomes the results are less clear cut. In all cases, except for the share of expenditure dedicated to education, the inclusion of the pre-1890 colonies reduces the size of the slavery coefficient and of the first stage F-stat. For the share of public services and for public services expenditure per capita, the inclusion of pre-1890 colonies also renders the coefficient insignificant. However, the coefficient on the colonies itself is insignificant for the public service share. For per capita education expenditure, the estimates are essentially unchanged by the inclusion of the pre-1890 colonies, suggesting a different channel than that of immigrant settlement for the impact of slavery. This will be explored in future research.

Table 5.9: The settlement channel, IV estimation

Panel A: Second Stage												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	RevPerCap	RevPerCap	ExpPerCap	ExpPerCap	PubServShare	PubServShare	PubServPerCap	PubServPerCap	PubEduShare	PubEduShare	PubEduPerCap	PubEduPerCap
SlaveRatio1872	-4.875	-70.90**	-18.47	-69.81**	-1.564	-2.370**	-9.776	-41.35**	-0.229	-0.113	-2.113**	-2.304**
	(25.13)	(33.20)	(23.10)	(32.15)	(1.157)	(1.191)	(17.78)	(20.24)	(0.178)	(0.171)	(1.034)	(1.078)
ColoniesPre1890	5.768***		3.703***		0.0793		3.106***		-0.0114		0.0188	
	(1.492)		(0.920)		(0.0606)		(1.044)		(0.00785)		(0.0465)	
Colonies1890-1912	0.789	2.231*	1.840*	2.771**	-0.0156	0.00132	-0.326	0.335	0.0131	0.0107	0.0881**	0.0921**
	(1.131)	(1.310)	(1.037)	(1.353)	(0.0508)	(0.0529)	(0.532)	(0.716)	(0.00830)	(0.00818)	(0.0399)	(0.0413)
ColoniesPost1912	-0.336	0.338	-0.659	1.139	0.0138	0.0280	-0.267	0.288	0.00835	0.00630	0.0238	0.0271
	(1.229)	(1.574)	(1.084)	(1.653)	(0.104)	(0.126)	(0.844)	(1.636)	(0.0127)	(0.00998)	(0.0881)	(0.0945)
Constant	48.00*	125.3***	72.54***	130.3***	1.050	2.031	36.37	74.76***	0.493**	0.352*	4.755***	4.987***
	(28.35)	(40.15)	(25.43)	(39.57)	(1.830)	(1.891)	(25.73)	(28.34)	(0.213)	(0.193)	(1.262)	(1.298)
Additional controls												
Geo variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Land suitability	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
1836 variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
1872 variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	171	171	171	171	159	159	159	159	159	159	159	159
R-squared	0.584	0.010	0.494	-0.030	0.041	-0.141	0.257	0.031	0.053	0.144	0.084	0.017
Panel B: First Stage												
F-stat	9.38***	10.34***	9.38***	10.34***	9.12***	11.49***	9.12***	11.49***	9.12***	11.49***	9.12***	11.49***
Part R-squared	0.0490	0.0437	0.0490	0.0437	0.0554	0.0577	0.0554	0.0577	0.0554	0.0577	0.0554	0.0577
Observations	171	171	171	171	159	159	159	159	159	159	159	159

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Revenue and expenditure per capita are 1908-1912 averages, the other outcome variables are measured in 1912. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Santos is excluded from the estimation. I drop the non white share variable and the coffee and sugar production variables in order to preserve degrees of freedom. They are insignificant and do not change the results qualitatively or quantitatively. I also drop longitude because, while not statistically significant, it is closely related to my instrument, and decreases its strength when included. All other controls included in previous estimations remain.

5.5.5 Adding Rio de Janeiro

As discussed above, Rio de Janeiro has a very different history from that of São Paulo. The city of Rio de Janeiro became Brazil's colonial capital in 1763 and in 1808 the Portuguese court moved there to escape Napoleon's invasion of Portugal. After Independence, the city became the capital of the new nation and remained so until it was replaced by the newly-contructed Brasília in 1960. The capitancy/province of Rio de Janeiro was home to a substantial production of sugar cane during colonial times, and was the birthplace of large-scale coffee production in Brazil in the early 19th century. Moreover, Rio was the main port of entry of slaves into Brazil in the 1800s. Although coffee production in Rio experienced stagnation and decline after 1850, the combination of all these factors meant that Rio de Janeiro had far more slaves in relative terms than São Paulo throughout history.

Table 5.10: Rio de Janeiro and São Paulo, standard and spatial IV estimation

	(1)	(2)	(3)	(4)
	IV	IV	SARAR IV	SARAR IV
VARIABLES	RevPerCap	ExpPerCap	RevPerCap	ExpPerCap
SlaveRatio1872	-63.22*	-55.29*	-13.78**	-13.85**
	(35.47)	(31.65)	(6.697)	(5.950)
Constant	71.77**	67.20***	15.00	10.93
	(28.41)	(23.15)	(17.98)	(13.28)
Lambda			0.765***	0.945***
			(0.269)	(0.264)
Rho			-0.0592	-0.469
			(0.677)	(0.832)
Additional controls				
Santos dummy			✓	✓
Rio de Janeiro dummy	✓	✓	✓	✓
Geo variables	✓	✓	✓	✓
Land suitability	✓	✓	✓	✓
1872 variables	✓	✓	✓	✓
First stage F-stat	8.85**	8.85**		
First stage part R^2	0.0223	0.0223		
Observations	219	219	220	220
R-squared	-0.226	-0.295		

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Lambda is the spatial lag and rho is the spatially weighted error term. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Revenue and expenditure per capita are 1908-1912 averages. Santos is excluded from the estimation in columns 1 and 2. The 1872 controls and other variables are those used in previous estimations. Population density is excluded from the last two columns due to missing values.

Rio de Janeiro was different from São Paulo also from an administrative point of view,

mainly because of its earlier settlement and smaller geographical extension. This is evident from the fact that, while over half of São Paulo's municipalities in 1912 were created after 1872, in Rio this figure was less than one third. Furthermore, in subsequent decades, the creation of new administrative units continued at a much faster pace in São Paulo than in Rio, particularly in the former state's western frontier regions. Rio's municipalities also tended to be much larger than São Paulo's. In 1872, the former had on average over 25,000 inhabitants while the latter only had 9,000. By 1912, this gap had substantially, but not completely, narrowed.

I show the outcome of including Rio de Janeiro's municipalities in the analysis in Table 5.10. For this exercise, I cannot include the 1836 controls and I can only look at the fiscal capacity outcomes, since the 1836 controls and information on public goods expenditure are not available for Rio. In order to account for structural differences across the two states, I add state fixed effects in the regressions.

I find remarkably similar results to those of São Paulo in isolation. Using both standard IV and SARAR IV models, I find a strongly negative effect of slavery on fiscal capacity. Similar results also emerge using 3SLS. Once again, coefficients are smaller when I use my spatial model, but the qualitative result is unchanged. The negative impact of a 10 percentage point increase in the slave ratio can be quantified with a 6.3-1.4 milreís drop in per capita revenue and 5.5 -1.4 milreís drop in per capita expenditure. These are large numbers given that, excluding Santos, average revenue and expenditure per capita in the two states were 5.1 and 4.3 milreís respectively in 1908-12.

The results also confirm the existence of large positive spillover effects between neighboring municipalities. This suggests the existence of strategic complementarities in decisions regarding the development of fiscal capacity at the local level.

5.6 Conclusion

Slavery has long being singled out as one of the determinants of economic underperformance in both sending and receiving countries. Despite this, no conclusive evidence of the impact of slavery on broad economic and institutional outcomes in Brazil, the largest importer of slaves during the Atlantic slave trade, exists. In this paper, I provide evidence that slavery negatively affected the development of fiscal capacity and public goods provision in Brazil, by analyzing municipal level outcomes in Rio de Janeiro and São Paulo.

Although the raw data and standard regressions are inconclusive regarding the relationship between slavery and fiscal capacity, in line with previous work, I show that this is

due to the existence of multiple sources of bias. Once these are accounted for using spatial models and an instrumental variable strategy, the strongly negative effect of slavery on the development of fiscal capacity and on the provision of public goods provision in Rio de Janeiro and São Paulo's municipalities emerges clearly.

Appendices

5.A Additional analysis and robustness checks

In this section, I present some further estimations, which illustrate the robustness of the results of the paper. I have performed many additional robustness checks not shown here in the interest of space. However, one of these deserves to be mentioned.

I have assessed the robustness of my results in relation to the other existing study, which offers clear cut findings regarding the effects of slavery on developmental outcomes in Brazil. Fujiwara, Laudares, and Valencia Caceido (2017) show that municipalities on the Portuguese side of the line drawn as a result of the Treaty of Tordesillas had more slaves than those on the Spanish side in 1872 and use this fact in a regression discontinuity framework to test the long-run legacy of slavery on inequality and other outcome variables. I employed the distance from the Tordesillas line interacted with a dummy indicating whether a municipality was on the Portuguese or Spanish side as an instrument for the incidence of slavery and found that this was only weakly related to slave ratios in São Paulo. The inclusion of controls eliminated even this weak correlation. I also tested the sensitivity of my results to adding these variables directly in the regression in an attempt to capture any residual differences between municipalities on either side of the Tordesillas line not accounted for by my controls, but found the results to be completely unchanged.

The first robustness check I present here is the "neighbor" style estimation described in Section 5.5.3 (Table 5.11). As suggested by Gibbons and Overman (2012) to overcome the endogeneity issues connected with SARAR models, I introduce the spatial lag of the explanatory variables as controls rather than the spatial lag of the depednet variable. Additionally, I use the spatial lag of the main variable of interest – the 1872 slave ratio – instead of the variable of interest itself. This is to minimize any remaining worries of endogeneity. The spatial lag of the slave ratio is furthermore instrumented using the spatial lag of the instrument. The first stage F-stats are very large, revealing that the instrument works even better by averaging over large geographical areas.

The impact of slavery on revenue and expenditure per capita and on the education outcomes is statistically significant and strongly negative, confirming the findings above. The coefficients are negative also for the public services outcome variables, but not measured precisely enough to be statistically significant at conventional levels. This is not particularly worrying given that the three stage least square estimation presented in the body of paper recovers a significantly negative effect of slavery on these variables as well.

In this estimation, the lack of significance is very probably due to the low number of degrees of freedom left given the large number of controls utilized.

Table 5.11: "Neighbour" IV estimation

Panel A: Second Stage						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	RevPerCap	ExpPerCap	PubServShare	PubServPerCap	PubEduShare	PubEduPerCap
SL_SlaveRatio1872	-510.3***	-408.6***	-9.149	-178.1	-2.458**	-19.72***
	(153.4)	(133.7)	(8.512)	(112.3)	(1.129)	(5.500)
Constant	40.32	43.47	-0.209	30.85	-0.0661	1.432
	(34.81)	(28.79)	(2.244)	(31.04)	(0.226)	(1.164)
Additional controls						
Geo variables	✓	✓	✓	✓	✓	✓
Land suitability	✓	✓	✓	✓	✓	✓
1836 variables	✓	✓	✓	✓	✓	✓
1872 variables	✓	✓	✓	✓	✓	✓
Spatial lag of controls	✓	✓	✓	✓	✓	✓
Observations	171	171	159	159	159	159
R-squared	-0.014	0.040	-0.148	0.0284	0.128	-0.015
Panel B: First Stage						
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	SL_SlaveRatio1872	SL_SlaveRatio1872	SL_SlaveRatio1872	SL_SlaveRatio1872	SL_SlaveRatio1872	SL_LagSlaveRatio1872
Instrument & other variables of interest						
SL_LN(CoffeeSuit*DistPort)	0.122***	0.122***	0.125***	0.125***	0.125***	0.125***
	(0.159)	(0.159)	(0.177)	(0.177)	(0.177)	(0.177)
SL_DistPort	-0.000857***	-0.000857***	-0.000841***	-0.000841***	-0.000841***	-0.000841***
	(0.000153)	(0.000153)	(0.000162)	(0.000162)	(0.000162)	(0.000162)
SL_CoffeeSuit	-0.000393***	-0.000393***	-0.000388***	-0.000388***	-0.000388***	-0.000388***
	(0.0000926)	(0.0000926)	(0.0000962)	(0.0000962)	(0.0000962)	(0.0000962)
F-stat	59.17***	59.17***	49.9***	49.9***	49.9***	49.9***
Part R-squared	0.333	0.333	0.352	0.352	0.352	0.352
Robust standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

SL stands for spatial lag. Revenue per capita is 1908-1912 average. Santos is excluded from the estimation. In the IV estimation I drop the non white share variable and the coffee and sugar production variables. They are insignificant and does not change the results qualitatively or quantitatively. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Revenue and expenditure per capita are 1908-1912 averages. The 1872 controls and other variables are those used in previous estimations.

The next robustness check consists in omitting sources of revenue that might affect my results from the revenue per capita measure. More precisely I omit state subsidies and borrowing. Both the data and the historiography do not offer much evidence on the mechanisms with which government subsidies were assigned. Some municipalities received none, while other received substantial amounts, with no consistency over time. Neither does it appear that poorer municipalities received more subsidies. If anything, municipalities with higher non-subsidy revenues per capita tended to receive more help from the government. This suggests that subsidies might have been tied to political rather than economic considerations. It could also mean that they satisfied temporary funding needs connected, for example, to large infrastructure investments, such as the installation of sewers or public lights. The erratic and apparently arbitrary nature of the subsidies

makes their relationship with fiscal capacity unclear. Did subsidies substitute and prevent fiscal capacity accumulation? Did municipalities endowed with adequate fiscal capacity not exploit it because the could rely on transfers from the state instead? Were the subsidies an alternative to borrowing for capital investments and other revenue shortfalls?

The relationship between borrowing and fiscal capacity is less controversial, but still not entirely straightforward. Before World War II, it was commonplace to classify borrowing as part of public revenues, often in a separate extraordinary revenue section of public accounts. Today, instead, borrowing is considered as a separate item in public accounts. This is because the proceeds of borrowing reflect funds that will have to be repaid, but also because it is unclear to which fiscal year the "revenue" coming from loans should be assigned to. So, should loans be considered part of a government's fiscal capacity? O'Brien (2011) suggests that they should. This is reasonable in light of the intimate relationship between fiscal capacity and borrowing capacity (see Chapter 4). However, the issue of how to assign loans to fiscal years remains.

These considerations indicate that there is no clear way in which the inclusion of the two revenue items should lead to bias. It appears safe to assume that subsidies and loans are not connected with the legacy of slavery though a channel other than fiscal capacity. In case this assumption were violated, estimates of the impact of the institution might be biased either upwards on downwards. Given these complex considerations, I have left the two revenue items in my main analysis, reassured by the fact that my other outcome variables will reliably capture the two aspects of fiscal capacity I am interested in: the share of resources commanded by a municipality, and its allocation to public services. To dispel any worries of bias coming from loans and subsidies, I also re-run my analysis excluding these items using standard IV, SARAR IV, "neighbor" IV and 3SLS methods (Table 5.12). The estimations yield results that are consistent with my previous analysis both qualitatively and quantitatively. Only the standard IV yields an insignificant albeit still negative coefficients. Thus, the strongly negative impact of slavery on fiscal capacity development emerges clearly from this excercise as well.

Moreover, I also find evidence of positive spillover effects, which were absent in previous estimations for revenue per capita (excluding the specification when Rio de Janeiro is included in the estimation). This suggests that the variable excluding loans and grants might, indeed, represent a cleaner measure of fiscal capacity.

The final robustness check regards the immigrant settlement channel. Table 5.13 shows that when I use the SARAR IV estimator results are broadly consistent with the standard

Table 5.12: Revenue per capita excluding loans and government grants, standard and spatial IV estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	IV	SARAR IV	"neighbor" IV			3SLS	
VARIABLES	RevPerCapAlt	RevPerCapAlt	RevPerCapAlt	RevPerCapAlt	SlaveRatio1872	SL_RevPerCapAlt	SL_SlaveRatio1872
SL_RevPerCapAlt		0.641*		0.729*			
		(0.328)		(0.415)			
SlaveRatio1872	-36.90	-10.05*		-18.18**			
	(28.04)	(5.808)		(8.819)			
SL_SlaveRatio1872			-377.0***			-25.18***	
			(107.6)			(5.551)	
LN(CoffeeSuit*DistPort)					0.0565***		
					(0.0212)		
DistPort	-0.0202**	-0.000113	0.0179	0.00118	-0.000592***		
	(0.00966)	(0.00697)	(0.0112)	(0.00884)	(0.000136)		
CoffeeSuit	0.0129**	0.00998*	0.00625	0.0120**	-0.000102		
	(0.00636)	(0.00578)	(0.00500)	(0.00560)	(0.000105)		
SL_LN(CoffeeSuit*DistPort)							0.108***
							(0.0121)
SL_DistPort			0.0958*			-0.00192	-0.000793***
			(0.0556)			(0.00394)	(0.000101)
SL_CoffeeSuit_spl			0.0518			0.0294***	-0.000319***
			(0.0599)			(0.00370)	(8.65e-05)
Rho		-1.248					
		(1.182)					
Constant	76.09**	26.16*	-10.88	18.66	0.614**	-0.107	0.00355
	(32.43)	(14.84)	(24.99)	(21.89)	(0.281)	(0.153)	(0.00242)
Additional controls							
Geo variables	✓	✓	✓	✓	✓	✓	✓
Land suitability	✓	✓	✓	✓	✓	✓	✓
1836 variables	✓	✓	✓	✓	✓	✓	✓
1872 variables	✓	✓	✓	✓	✓	✓	✓
Spatial lag of controls			✓			✓	✓
First stage F-stat	10.91***		49.9***				
Observations	159	160	159	160	160	160	160
R-squared	0.388		0.612	0.660	0.696	0.980	0.997

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

SL stands for spatial lag. Lambda is the spatial lag and rho is the spatially weighted error term. Santos is excluded in columns 1 and 3 due to issue of singularity of matrix. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Revenue per capita are 1908-1912 averages. Lambda is the spatial lag and rho is the spatially weighted error term. The 1836 and 1872 controls and other variables are those used in previous estimations.

IV estimation. Using three 3SLS yields very similar results, but I refrain from reporting it to avoid the further proliferation of regression tables.

For revenue per capita, the introduction of the pre-1890 colonies dummy reduces substantially the magnitude of the slavery coefficient from -20 to -13. However the coefficient remains significantly negative even when the dummy is included, indicating that the settlement channel might not be the only way slavery shaped local institutions. For expenditure, instead, the results are qualitatively very similar to the standard IV estimation in that the slavery coefficient becomes insignificant when the pre-1890 colonies dummy is included. The same is true for the public services per capita outcome. In all these cases, the pres-

ence of the 1890-1912 and the post 1912 placebo dummies does not affect the impact of slavery, in line with my hypothesis. As for the standard IV estimation, the SARAR estimates indicate the settlement channel was not the way in which slavery negatively affected expenditure on municipal education. For the public service and education shares this exercise is not very informative, as the negative impact of slavery on these outcomes is not evident either with or without the pre-1890 colonies, in contrast to the more robust 3SLS results. This suggests different underlying channels for the effect of slavery, but might also reflect missing observations in the additional outcomes.

Table 5.13: The settlement channel, SARAR IV estimation

VARIABLES	(1) RevPerCap	(2) RevPerCap	(3) ExpPerCap	(4) ExpPerCap	(5) PubServShare	(6) PubServShare	(7) PubServPerCap	(8) PubServPerCap	(9) PubEduShare	(10) PubEduShare	(11) PubEduPerCap	(12) PubEduPerCap
SlaveRatio1872	-13.22**	-19.98***	-10.09	-14.44**	-0.0749	-0.217	-6.463	-7.404**	-0.0600	-0.0569	-0.601***	-0.584**
	(6.637)	(7.207)	(6.139)	(6.634)	(0.337)	(0.355)	(4.082)	(3.688)	(0.0498)	(0.0479)	(0.233)	(0.233)
ColoniesPre1890	6.205***		3.905***		0.0968*		1.867		-0.00671		0.00440	
	(1.544)		(0.892)		(0.0580)		(1.387)		(0.00786)		(0.0363)	
Colonies1890-1912	1.401	0.314	2.070**	1.391	-0.0565	-0.0552	-1.107*	-1.194**	0.00948	0.00961	0.0309	0.0302
	(1.014)	(1.582)	(0.956)	(1.130)	(0.0412)	(0.0412)	(0.589)	(0.594)	(0.00725)	(0.00724)	(0.0317)	(0.0317)
ColoniesPost1912	-0.291	0.951	-0.369	0.374	-0.0241	-0.0277	-0.221	-0.390	0.00908	0.0102	0.00465	0.00528
	(1.310)	(1.134)	(1.083)	(0.962)	(0.0922)	(0.0907)	(1.091)	(1.119)	(0.0110)	(0.0113)	(0.0496)	(0.0496)
Constant	47.26*	41.73	41.17*	38.35	-0.678	-0.226	-2.614	1.168	0.169	0.133	1.422*	1.363*
	(24.74)	(33.75)	(23.15)	(26.40)	(1.241)	(1.271)	(14.24)	(16.08)	(0.144)	(0.142)	(0.740)	(0.749)
Lambda	0.276	0.543	0.543	0.724*	-0.109	-0.205	0.583	0.348	0.917	1.133	0.768	0.807*
	(0.320)	(0.395)	(0.360)	(0.395)	(0.442)	(0.442)	(0.718)	(0.749)	(0.713)	(0.737)	(0.479)	(0.483)
Rho	-3.545**	-1.358	-2.063	-1.043	-0.377	-0.0506	-2.302	-1.727	-2.815	-3.287*	-1.798	-1.749
	(1.501)	(1.285)	(2.022)	(1.711)	(0.998)	(0.952)	(1.866)	(1.842)	(1.936)	(1.964)	(1.615)	(1.604)
Additional controls												
Santos dummy	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Geo variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Land suitability	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
1836 variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
1872 variables	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	173	173	173	173	161	161	161	161	161	161	161	161

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

SL stands for spatial lag. Lambda is the spatial lag and rho is the spatially weighted error term. Revenue and expenditure per capita are 1908-1912 averages, the other outcome variables are measured in 1912. Geographic controls include latitude and distance from the port of Santos or Rio de Janeiro (whichever is closer). The land suitability indicators refer to rain-fed, low input potential yields of coffee, sugar cane and cotton. Revenue per capita are 1908-1912 averages. Lambda is the spatial lag and rho is the spatially weighted error term. The 1836 and 1872 controls and other variables are those used in previous estimations.

5.B Variable definition and sources

Throughout the paper, I provide information on the sources of the data. In the text, I also outline how I deal with the creation of new municipalities after 1872, as well as any additional data issues. In this Appendix, I describe in more detail how the variables used in the estimation are constructed. Unless otherwise stated, all the 1872 data are from that year's census (Brazil, 1876) and refer to both Rio de Janeiro and São Paulo. The 1836 data comes from Bassanezi (1998), which in turn relies on Müller (1838), and is for São Paulo only.

Revenue and expenditure per capita: refer to the public revenue and expenditure of the municipalities of Rio de Janeiro and São Paulo normalized by the size of the population for the respective years. I collect these variables for five years – 1908-1912 – from the 1908-12 Brazilian statistical yearbook (Brazil, 1908-1912) and calculate averages to smooth out idiosyncratic annual variations.

Composition of revenue and expenditure: I collect this information for São Paulo in 1912 from the state's statistical yearbook from the same year (São Paulo, 1912). The data are more detailed than in the Brazil-wide yearbook, and thus provide a more complete picture of the municipalities' public finances. Revenues are classified as follows. Ordinary revenue: any remaining positive balance from the previous exercise, tax on industries and professions, property tax, transportation taxes, tax on coffee trees, taxes on water, sewer taxes, income of the cemetery, income of the slaughterhouse, income from the market, income from public lighting, recovery of active claims. Extraordinary revenue: deposits and cautions, state subsidy, loans obtained in the fiscal year. The components of expenditure are as follows. Ordinary expenditure: public works, street cleaning, public lighting, public health, market, cemetery, slaughterhouse, public water, public sewers, public education, wages and subsidies of municipal workers, office and publication expenses of the municipality, judicial expenses, extraordinary expenses, other expenses. Extraordinary expenditure: refunds and and returned deposits.

Using these data, I construct my additional outcome variables. The public service share is simply the share of expenditure on items clearly identifiable as public goods or other public services in total municipal expenditure. These services are: public works, street cleaning, public lighting, public health, market, cemetery, slaughterhouse, public water, public sewers, public education. The pubic services per capita measure is the expenditure on these same services normalized by population size. The public education

variables single out the, arguably, most important public good provided by municipalities in this period normalized by total public expenditure and population size respectively.

For my robustness checks I also construct a further revenue outcome variable. This is total revenue per capita excluding state subsidies and loans. I exclude the former because they may or may not reflect local level fiscal capacity (I discuss this issue in detail in the previous Appendix). I exclude loans because borrowing is not a revenue in a strict sense, and because it is unclear to which fiscal year the loans should be assigned and no information is available regarding maturities.

Slave ratio: is simply the share of slaves in the population of each municipality. I calculate this for 1872 and 1836.

Population size: is the number of people living within a municipality. I calculate this for 1872 and 1836. For 1872, I also calculated a quadratic term to account for non-linearities such as Zipf's and Gibrat's Laws (Gabaix, 1999).

Population density: is the number of people living within a municipality normalized by the surface area of that same municipality. I calculate this for 1872.

Initial state capacity: I construct two variables to proxy initial state capacity, one for 1836 and one for 1872. The 1836 variable is the number of tax collectors in each municipality normalized by population size in 1836. The 1872 variable is the number of public workers residing in a municipality normalized by population size in 1872.

Sugar and coffee production: This is the production in weight of these two commodities in 1836 at municipal level normalized by population size in the same year.

Literacy: this variable measures the share of the population who is reported as able to read and write. I calculate this for 1872.

New Municipality: is a dummy indicating whether a municipality was created after 1872.

Domestic migrants: is the share of Brazilian citizens in each municipality not born in the state where they currently reside. I calculate this for 1872.

Foreign migrants: is the share of citizens living in a municipality born outside of Brazil excluding slaves. I calculate this for 1872.

Share of agricultural workers: is the share of citizens of the municipality working in the agricultural or pastoral sector. I calculate this for 1872.

Non white share: the 1872 census divided the Brazilian population into four self-reported categories: brancos (whites), pardos (mulattoes), pretos (blacks) and cablocos (mestizios). I calculate the share of non whites in the population of each municipality

using all non-white categories.

Ethnic fractionalization: I calculate the degree of ethnic fractionalization in each municipality using the the index proposed by Alesina, Baqir, and Easterly (1999). The index's formula is:

$$EthnicFrac = 1 - \sum_{i=1}^{4} Ethnicity_i^2$$

where *Ethnicity* corresponds to share of the population belonging to each of the four self-reported categories outlined above.

Geographical controls and land suitability: I construct these variables based on municipal borders in 1911 as per the maps provided by the Instituto Brasileiro de Geografia e Estatística (IBGE, 2011). All variables are municipal averages. Latitude, longitude and distances refer to the centroid of each municipality and are as the crow flies distances which take into account the earth's curvature. The raw altitude data in 1km by 1km cells is from WorldClim (www.corldclim.org). The land suitability data are from the Food and Agriculture Organization (2012) and are based on monthly statistics of climatic variables and precipitation for the period 1960-90, collected in 10-30 arc minutes cells.

5.C Images

(a) Slaves on their way to work on the fields in Rio de Janeiro, 1858

(b) Coffee plantation in the settler colony of Conde do Parnahyba in Mogi Mirim - São Paulo, undated

Figure 5.13: Slaves and colonists

Image (a) is a lithography held at the Acervo Banco Itaú in São Paulo based on a photograph by Victor Frond held at the Biblioteca Nacional in Rio de Janeiro. Image (c) is from the digital archive of the Museu da Imigração do Estado de São Paulo

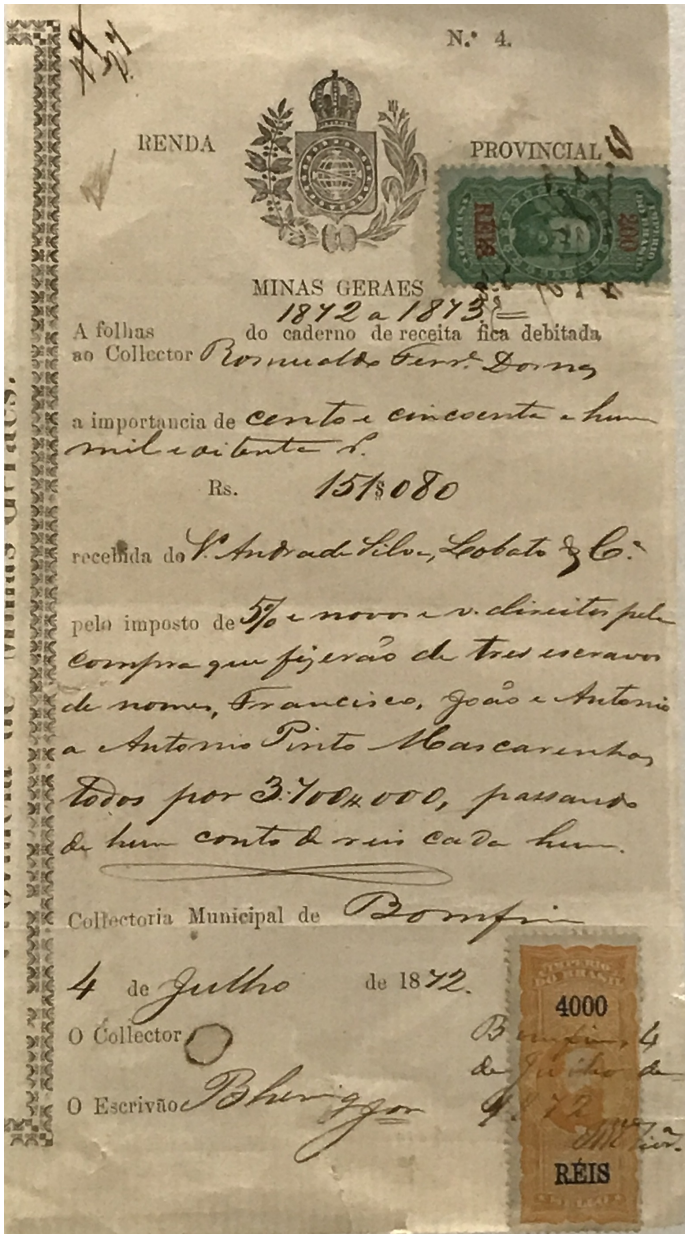


Figure 5.14: Tax paid on a transaction involving slaves (*meia siza*) in Bonfin - Minas Gerais, 4 July 1872

The price of the sale of three slaves – Antonio, João and Francisco – to Antonio Pinto Mascarenhas amounted to 3100 milreís while the tax paid was 151.08 milreís, curiously about 44 milreís less than the 5% tax rate would imply. Source: Acervo Banco Itaú, São Paulo

Chapter 6

Conclusion

In this concluding chapter, I will not repeat the points made in other parts of this Dissertation. I have laid out the general motivation and contribution of this work in the introduction (Chapter 1). I have reviewed the vast literature that has informed this work, and illustrated how this research helps to fill some of the existing gaps in our understanding in Chapter 2. In the three research papers, I have outlined in even more detail the motivation and contribution of this work (Chapters 3- 5).

Below, I will only briefly summarize some general lessons and ideas, which I believe the Dissertation raises or supports. The main discussion will deal with potential future avenues for research.

6.1 General lessons

This Dissertation has offered a new perspective on the origin and impact of a key institutional characteristic of countries: fiscal capacity. In the three papers that make up this dissertation, I have shown that:

1. Fiscal capacity is not only important for long term development, but also in short term and cyclical outcomes. More generally, my findings support the idea that deeply-rooted fundamentals and institutions can shape how countries react to shocks and constrain the available policy options.

   This is demonstrated by the fact that more fiscally fragile countries were much more likely to default during the Great Depression and by the fact that the path of their public revenues was strongly influenced by the level of fiscal development they possessed at the even of the slump. These findings highlight the importance of incorporating institutional constraints in both historical research on the Great De-

pression (and beyond) and in theoretical research in macroeconomics and finance. Although some steps in this direction have been taken in recent years, most macroeconomic models still ignore the fact that states are not always *effective* and that their capacities might be severely limited by their institutional development.

Regarding research on the Great Depression, while much work has been dedicated to the impact of Gold Standard membership and the influence of WWI in upsetting the balance of power within countries, I argue that it needs to be recognised that the Depression hit countries possessing vastly different and deeply-rooted institutions. This not only shaped the policy choices available to them, but also determined the outcome of these policies. This is most evidenvet in the fact that, after leaving the Gold Standard, weak countries saw their borrowing costs rise, while fiscally strong once experienced lower borrowing costs.

2. Fiscal capacity is a useful and comprehensive measure of institutional quality.

   While this is the central tenet of the literature on state formation and state capacity, we still lack much direct empirical evidence that this is the case. Research has highlighted the existence of a positive correlation between the ability to tax and important outcomes, such as income per capita and a low incidence of civil conflict. Some studies have attempted to establish a causal link between these variables. However, the channels and exact mechanisms though which fiscal capacity translates into better economic performance and less conflicts are not yet entirely clear. I show that fiscal capacity gave countries an easier access to borrowing and reduced the likelihood of default. Both these factors can play an important role in the long-term health of countries by allowing them to smooth public revenues, reduce macroeconomic and political volatility and provide public goods consistently. Thus, the intimate link between fiscal capacity and borrowing, might be one of the channels through which fiscal development leads to better economic outcomes.

3. The development of fiscal institutions takes place though to complex political economy mechanisms, which go beyond the warfare-state formation nexus discussed in much of the literature.

   In this work I have illustrated two such mechanisms. First, I have argued that the incidence of natural disasters can act as a constraint to the development of centralized fiscal institutions. This is because most disasters tend to be small, and because specific disaster types tend to affect specific areas repeatedly. This means that

specialized local level relief provision can be more effective than centralized efforts. Moreover, local level disaster relief raises the heterogeneity of preferences regarding public goods, and is also a public good with little spillover effects. This makes resource mutualization more difficult and facilitates local compared to centralized provision. Second, I have shown that slavery damaged the development of local fiscal institutions and public goods provision in Brazil. Part of this effect was due to the interaction between slavery and mass migration. By discouraging the settlement of foreign migrants which could count on a disproportional political weight, slavery nagatively affected the development of fudamental ingredients for long term economic growth.

6.2 Future work

While helping to answer some questions, this Dissertation has also raised many new ones and highlighted the need for further work in some areas. Below, I illustrate what I believe to be some promising avenues for research.

With regards to my paper on the Great Depression debt crisis, one of its most obvious extensions is to go beyond defaults on dollar denominated bonds and analyze defaults on debts denominated in pounds, francs and other currencies. Although US lending was dominant in the interwar period, a holistic picture of the crisis would reinforce the findings of my study. The main challenge for this exercise is identifying detailed sources on outstanding debts and default dates for both central and local governments to construct the outcome variables.

My expectation is that the main result of the analysis will be very similar to my findings for dollar-denomniated debts, since, in the vast majority of cases, defaults involved the debts of countries in all currencies. However, the fact that British and US creditors were sometimes treated differently in the interwar period due to the British governmnet's more interventionist attitude – the German default is a case in point (Eichengreen and Portes, 1988) – might lead to some variation and interesting political economy considerations. For example, it is possible that falling public revenues were less conducive to default on sterling bonds compared to dollar bonds, all else equal, because the former were prioritized. Such a finding would echo the result that liquidity constraints and gold reserves played a much bigger role in local defaults compared to central level ones, since scarce resources were allocated in a hierarchical way to debt repayments.

A second important offshoot of the my defaults paper would be a direct and explicit ex-

plorations of the fiscal limits of defaulters during the interwar years. Using recent advances in the literature on endogenous fiscal limits and asymmetric information discussed in the paper, it would be very valuable to attempt to assess how fiscal limits might have changed with the onset of the Depression. In particular, it would be extremely useful to investigate how the slump affected countries' ability to tax and service debts by concentrating on one or two country case studies.

With regards to the themes discussed in my second paper, I envision an exploration of the consequences of the dynamics of fiscal aggregates I describe. In the paper, I note that the early 1930s were rife with regime changes and a general increase in authoritarianism. Fiscal instability and outright crises might have been important contributors to both, if they affected the ability of states to function normally and provide public goods. Systematically investigating the link between the dynamics of public revenues and expenditure and political outcomes would be a very valuable exercise. The Great Depression offers the ideal backgound to do so due to the large degree of economic and political volatility. This analysis would also involve a simple theoretical framework on which I have already started working.

My paper on local fiscal institutions in Brazil warrants extensions across time and space. While it would be interesting to do the analysis for all of Brazil, this would require a dramatically different identification strategy. A less demanding extension would be to bring the state of Minas Gerais into the mix. This had a different colonial legacy and production structure from São Paulo and Rio de Janeiro and would thus provide a genuine test of external validity for my results. At the same time, the state also partook in the coffee boom and experienced a strong development in the 19th century. This would allow me to use the same or similar identification strategy to the one used for Rio de Janeiro and São Paulo. Similarly to São Paulo, Minas Gerais also appears to have rich municipal level sources dating from before 1872, which could be exploited construct a wide range of controls.

Regarding the time dimension, I would like to explore the persistence of my results to the present. Persistence papers are, by now, a staple in economic research. Investigating the persistence, or lack thereof, of the effects of slavery on local institutions and development more broadly would be a valuable exercise, given the deep further structural changes Brazil underwent in the 20th century.

An additional connected project would be a further analysis of the impact of migration on institutional development in Brazil. A promising strategy for this study would involve

exploiting an exogenous source of variation provided by a policy intervention by the Italian Government. The 1902 "Decreto Prinetti" outlawed subsidized emigration from Italy and was mainly targeted at migration to Brazil, due to worries regarding the conditions of Italian migrants, particularly colonists working on coffee plantations. In combination with pre-existing immigrant settlement patterns, this policy very probably affected the number of migrants to settle in each municipality. This is because migrants tend to cluster in national networks and settle where their predecessors settled. Therefore, using information about the number of Italian migrants of each municipality before the decree, one can estimate the expected number of new arrivals in the absence of the decree by using the emigration life-cycle theory, which posits that a country's total emigration will be a non-linear function of time (Hatton and Williamson, 1998). The number of "missing migrants" – the difference between the expected and actual numbers of migrants – would be the treatment effect. This approach is similar to the one Bin (2017) uses to gauge the impact of immigration quotas in the United States, but with the added benefit the decree was implemented by a foreign government and is therefore much more credibly exogenous to the conditions of Brazilian municipalities than the immigration quotas were to conditions across the US.

I look forward to the challenges of these new projects and hope to enjoy them as much as I enjoyed the four years spent working on this Dissertation.

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